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ORIGINAL COMMUNICATIONS.

ON SUPERFÆTATION.

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CASES have, from time to time, occurred, which have induced many physiologists to assert the possibility of a woman again conceiving after she has become pregnant. The phenomenon thus produced has received the name of SUPERFÆTATION; and, although rare, it is worthy of consideration, both from the interest which is attached to it as a physiological occurrence, and from the possibility of its giving rise to medico-legal questions, affecting the reputation or welfare of the individuals concerned. It is not my intention, however, to enter into any discussion on the latter subject; and I will therefore merely quote the words of Capuron in illustration of its possible importance. “Quoique cette question ne se présente pas fréquemment, elle peut être d’un grand intérêt sous le rapport de la médecine légale. Une veuve, après être accouchée d’un premier enfant, peut accoucher quelque temps après d’un second, dont les collatéraux contestent la légitimité, parce qu’ils n’ont aucune idée de la superfétation. Il peut arriver encore que cette veuve se remarie bientôt après ses relevailles, et qu’elle accouche, peu de temps après ce second mariage, d’un second enfant à terme et très-viable. Il s’agit alors de décider auquel des deux maris appartient le second enfant.”¹ I purpose then to confine my observations to the consideration of Superfætation as a physiological phenomenon. It will be impossible to entirely determine whether it actually took place in all the cases alleged to have occurred; but we may, from a careful examination of these cases, and an impartial consideration of the opinions of the various authors who have written on the subject, be able to draw some probable inferences, while we are at the same time enabled to see more clearly what additional data are required for the perfect comprehension of a subject so difficult of investigation.

The occurrence of cases which gave rise to the theory of Superfætation was not unnoticed by the ancients. A work *περὶ ἐπικύσεως* has

been ascribed to Hippocrates; Aristotle also, and Pliny, refer to the subject; and Zacchias, in his "*Quæstiones Medico-legales*", says that the occurrence of Superfœtation has been alluded to by the classic writers. In confirmation of this he quotes, among other instances, the narrative of the births of Hercules and Iphiclus from Hesiod; also the passage of Plautus—

"Nunc, de Alcumenâ ut rem teneatis rectiùs,
Utrinque est grâvida—et ex viro, et ex summo Jove."

It attracted a considerable degree of attention among the physicians who lived two or three centuries ago. One of these old writers, Brasavolus, mentions that he had seen Superfœtation *epidemic*! Haller, in his "*Elementa Physiologiæ Corporis Humani*", enters at some length into the consideration of the question of Superfœtation, and gives an epitome of the cases he had been able to collect. A very interesting and instructive treatise on the subject was published in 1738 by Gravel, in the form of an inaugural dissertation, entitled "*De Superfœtatione Conjecturæ*."¹ Since that period, Superfœtation has been referred to by most of the writers on obstetric and forensic medicine.

The most usual definition of Superfœtation, is *the occurrence of a new conception, when the cavity of the uterus is already occupied by an embryo*. If, however, we admit with Velpeau the possibility of such an event taking place in an extra-uterine pregnancy, it then becomes necessary to extend the definition, and to consider Superfœtation as *the fertilization of an ovum previously to the expulsion of the embryo which already exists within the body of the mother*.

The cases which I have been able to collect as a foundation for the remarks I purpose making, may be arranged under several heads. A division of the cases of Superfœtation into classes is found in Gravel's thesis. He specifies four kinds of Superfœtation: "1. *Ficta*; quæ aut secundum leges naturæ videtur impossibilis, aut certas ob rationes, cùm facta non sit, facta esse contenditur. 2. *Spuria*; quando uno tempore concepti gemelli simul lucem adspiciunt, aut ambobus, aut uno saltem perfectis existentibus, si nimirum placentæ adhæserint, vel utrasque in eodem utero senserit cauta et experta obstetricantis manus. 3. *Dubiam* ostendit hypothesis sequens: Excludit puerpera infantem omni numero perfectum, quem sponte secundinæ sequuntur; hunc statim, vel etiam modico quarundam horarum intervallo, alius fœtus, immaturus, paucarum ut videtur mensium, excipit. 4. *Veræ* denique superfœtationis idea erit, si mulier post legitimum partum et consuetum lochiorum fluxum, intercedente aliquot dierum, septimanarum, aut etiam mensium spatio, alio fœtu itidem maturo enititur." The third and fourth of these kinds are those most generally recognized. The first seems to be so rare as not to have been made the subject of special notice, while the second is nothing more than twin conception; for the fact of there commonly being a *distinct* placenta for each twin, generally united in one mass, sometimes separated by an interval, is recognized by most obstetric practitioners. May not all twin pregnan-

¹ This thesis is to be found in Halleri Disput. Anatom. Select., tom. v, p. 337. Gottingæ: 1750; also in Schlegel, Sylloge operum minorum præstantiorum ad artem Obstetricam spectantium, vol. i, p. 325. Lipsiæ: 1795.

cies be, in fact, cases of Superfœtation? This is an interesting subject for discussion; but I can merely allude to it here,

Velpeau says¹ that all the cases of Superfœtation can be referred to four divisions:—1. Twin pregnancies, in which one of the fœtuses has died at an early period, and has been retained in the uterus until the expulsion of the other at the full term of gestation. 2. Twins unequally developed, or born at different times. 3. Extra-uterine pregnancies, which have not prevented a natural gestation. 4. Cases occurring in double uteri. 5. He admits also a class of cases which he terms "*Superfœtation proprement dite*", comprising those in which children of different colours have been born.

Velpeau's classification appears very convenient for the consideration of the cases; I propose, then, to adopt it, with some modification, and to arrange the cases in the following order:—

1. Cases in which a dead twin has been retained in the uterus.
2. Cases in which children have been born either
 - (a) both at the same time, but of different degrees of development; the smaller fœtus being either alive, or very recently dead; or,
 - (b) where the less perfect fœtus has been retained till it became mature.
3. Cases in which children of different colours have been born.
4. Cases in which a double uterus has been discovered to exist.
5. Cases occurring during extra-uterine gestation.

I. The first class of cases, those in which a dead twin has been retained in the uterus, cannot, I think, be regarded otherwise than as very equivocal examples of Superfœtation. The following are some of the instances.

Baudelocque² relates the case of a woman, who, in the year 1772, had a severe fall when advanced four months and a half in pregnancy. This occasioned symptoms of threatening abortion, which were relieved by bleeding and rest. At the full term, she was delivered of two children, one of which was mature, while the other was dead, apparently about five months old; it had undergone scarcely any decomposition. The placenta is described as having been single. Baudelocque attributed the death to the fall.

Dr. Denman³ gives the case of a female, who went to the ninth month of pregnancy: but, between the fifth and sixth, she met with a great fright, which affected her greatly, and diminished her size. On the 11th of February, she was delivered of a healthy child, but continued in pain; and it was not till the morning of the 25th that she was relieved. On that day, there was born the head and parts of a child that had just the appearance of a miscarriage of four months.

Not to multiply cases of this kind in detail, I will refer the reader to Beck's *Medical Jurisprudence*, seventh edition, p. 159, where a number of references are given. That both the above-mentioned cases were not instances of Superfœtation, is evident from their history; for, in both, a distinct date is assigned to the occurrence of

¹ VELPEAU. *Traité complet de l'Art des Accouchemens*, tom. i, livre 4, chap. 3.

² BAUDELLOCQUE. *L'Art des Accouchemens*, tom. ii, p. 351. Paris: 1781.

³ DENMAN (Thomas, M.D.). *An Introduction to the Practice of Midwifery*.

symptoms denoting the death of one child, corresponding with its apparent age when born. A dead fœtus may be retained for several months without undergoing decomposition, or but very little; and Baudelocque's case is a good example of this. It may be urged, that this was an example of an arrested twin, and that death took place only a short period before its expulsion; but the coincident circumstances of the case scarcely warrant such a supposition. From the following case, it would even appear that the presence of a dead fœtus in the uterus may not be an obstacle to the conception of another, which may then be considered as a Superfætation.

Baron Percy¹ relates the instance of a woman, residing in the vicinity of Lagny, who became pregnant in July 1820, for the first time. She felt distinctly the movements of the child about the fourth month, which gradually became more feeble, and at last entirely ceased. In the course of seven weeks from this period, she experienced all the symptoms of a fresh pregnancy; and the nine months of this gestation passed on without any remarkable occurrence. She was attended by Dr. Cochard, of Lagny, and Dame Robert. She had an easy and rapid labour, and was delivered of a small, but lively male child. Soon afterwards, fresh pains came on, during which a number of black unorganized coagula escaped from the uterus, in the midst of which was a female fœtus, seemingly of the fourth month, and well preserved. The translator expresses it as his opinion, that the second conception took place after the death of the first child.

Haller refers to partial abortion. He states it to be certain that one twin may die, while the other remains in the uterus; and the possibility of such an occurrence is now generally allowed. We are, therefore, justified in rejecting from the catalogue of cases of Superfætation, nearly all those in which one of twins has died, and has been retained in the womb.

II. The second class of cases is more worthy of consideration, as they have mainly served as the foundation of the difference of opinion, which has prevailed regarding the possibility of Superfætation. It includes—(a) Cases in which two children, of different degrees of development, were born alive at the same time; and (b), Cases in which one was retained for some days, weeks, or even months after the other, until it had arrived at maturity. The following are examples.

(a) *Simultaneous birth of children of different degrees of development.*

Ventura² relates the case of a lady, who considered herself as being arrived at the ninth month of pregnancy. During the first four months, she had periodically a moderate sanguineous discharge from the uterus, without pain or indisposition; hence she doubted of being pregnant, until satisfied by the presence of abdominal enlargement and by the movements of the fœtus. As soon as this periodical evacuation ceased, she began to be troubled with nausea, vomiting, inappetency, lassitude, and other symptoms which had not previously affected

¹ Journal Universel, Mars 1823. Translated in *London Medical Repository*, vol. xx, p. 110. 1823.

² VENTURA, Don Joseph (Cirujano del Hospital General de la Pasion), *Preceptos Generales sobre las Operaciones de los Partos. Parte primera. Madrid: 1787.*

her. At the period of parturition, vigorous expulsive pains set in; and the feet of a child presented, apparently of about five or six months. It was baptized, and born alive. While employing friction over the abdomen, Ventura perceived still great size and hardness, and also the movements of a child. This was soon born, and was of the full size. There were two placentaë, corresponding in size with the respective fetuses.

The next case has some resemblance to that just related. Catherine Fournier Lafond,¹ aged 32 years, tall, thin, and well-formed; the catamenia have always been irregular. In June 1845, they ceased to appear, and certain symptoms induced her to think herself pregnant. At the end of August the menses reappeared on two occasions, at an interval of some days, but their duration was short, and quantity small. She vomited also at this time. The signs became more marked, and the catamenia ceased permanently. On the 28th of February, 1846, pains commenced, which continued all the following day and night. Towards morning, on the 2nd of March, they appeared to diminish. The neck of the uterus was greatly dilated, the waters were slowly discharged, and the uterus became very sluggish. The child presented in a good position. Two grammes of ergot of rye were administered, under the influence of which the pains returned, and a dead child, apparently at the full time, was born. The placenta was soon expelled. Nine hours after the accouchement, she perceived the sensation of a body passing through the vagina, without any pain or great effort. This was found to be a second fœtus, having all the characters of one from four and a half to five months old; with a chord, a placenta still bloody, and membranous envelopes, all perfectly intact and well preserved.

M. Roch-Farbès,² of Toulouse, relates the following case. Madame Noël was delivered of a child at full term: Another fœtus, apparently of about six months, was also expelled. It could not suck, but was kept alive a week by means of cow's milk.

Dr. Fahrenhorst³ gives the case of a woman, who became pregnant in September 1825. Her health was perfectly good during the period of pregnancy, and there was no appearance of menstrual discharge. On the 28th of April, 1826, she was delivered, in the middle of the day, of a fœtus, enclosed in its membranes. After its expulsion, another bag of membranes was perceived; and, during the night, this was expelled. It contained a fœtus, supposed to be of the fourth month. The first fœtus appeared to have been born at the full term.

Pertus⁴ saw a woman delivered of a three months' fœtus, soon after which a five weeks' embryo was expelled.

Cases of this kind might be multiplied; but in many, the difference in the age of the twins has not been so great as to lead to any other supposition, than that they were conceived at the same time, or within a few days of each other.

(b) *Retention of the second fœtus.*

Zacchias⁵ gives the case of a woman, whose husband lost his life in

¹ Gazette Medico-Chirurgicale, Avril 1846, quoted in *Monthly Journal of Medical Science*, vol. vi, p. 144. 1846.

² Recueil Periodique de la Société de Médecine de Paris, tom. v. p. 191.

³ Rust's Magazine, 1827: quoted in *Medical Gazette*, vol. i, p. 806. 1826.

⁴ Revue Médicale, Mars 1838.

⁵ ZACCHIAS. Consilia, No. 66.

a quarrel. Eight months after, she was delivered of a deformed child, which died in the birth. Her abdomen remained large, and it was suspected that another infant was contained in the uterus, but all efforts to remove it proved unavailing. A month and a day after, she was again taken in labour, and brought forth a perfect living child. The relations of the husband contested its legitimacy, on the ground that it was a Superfætation.

Professor Eisenmann¹ relates the following case. Marie-Anne Bigaud, aged 37, was delivered of a living male child, on the 30th of April, 1748. Her delivery was so rapid and favourable, that she was able to return in an hour from the house of the midwife who had attended her, to the military hospital at Strasburg, where she resided. In her two former confinements, the lochia had been abundant; but in this one there was no loss of blood or of fluid, except at the moment of delivery. Her breasts, though naturally large, gave her no trouble, and did not become filled with milk, so that she was obliged, at the end of a fortnight, to provide a nurse for the child. These circumstances, together with her having distinctly felt the movements of another fetus, and the appearance of symptoms similar to those she had before felt during pregnancy, led her to consult M. Leriche, chief surgeon of the hospital, who ascertained, by manual examination, that she was some months pregnant. On the 16th of September, she was delivered of a living female child, evidently at the full period; after this, she had considerable hæmorrhage, and milk appeared in abundance. The first child lived only two months and a half; the second, a year and two days. After having borne several other children, Marie Bigaud died, in 1755, of an acute disease. Professor Eisenmann made a *post mortem* examination of her body; and, having twice met with double uteri, thought that a similar conformation might exist in this case. The uterus was, however, found to be simple.

Foderé² gives an account of a similar case:—Benoîte Franquet,³ of Lyons, was delivered of a female child on the 20th of January, 1780, and experienced, after her accouchement, a similar absence of milk and lochia to M. Bigaud. In three weeks, she felt the movements of a fetus; and her view was confirmed by M. Desgranges, whom she consulted. On the 6th of July, five months and sixteen days after the first, she was delivered of a second full-sized and healthy child. This accouchement was followed by the usual flow of milk and lochia. The children both lived; and, two years after, she made an affidavit of the fact before MM. Caillat and Dusurgey, notaries at Lyons, “not only to testify her gratitude to M. Desgranges, but also to furnish those women who might happen to be in a similar condition, and whose husbands died before the birth of the two infants, with a title in favour of their virtue and of the legitimacy of the second child.”

Dr. Maton⁴ published the following case:—Mrs. T., an Italian lady,

¹ De Utero Duplici, etc, auctore G. H. EISENMANN, Anatomix et Chirurgix Professore. Argentorati: 1752. This case is quoted in most of the works which treat on Superfætation.

² FODERÉ. Traité de Médecine Légale et d'Hygiène Publique. 2nd Edition. Paris: 1813. Vol. i, pp. 484-86.

³ This is evidently the same woman who is described in some accounts as “the wife of Raymond Villars.”

⁴ Transactions of the Royal College of Physicians, vol. iv, p. 161.

but married to an Englishman who was attached to the Commissariat of the British army in Sicily, was delivered, on the 12th of November, 1807, of a male child, which had every appearance of health. It was brought forth under circumstances very distressing to the parents, being dropped in a bundle of straw, at midnight, in an uninhabited room; and it survived nine days only. On the 2nd of February, 1808 (not quite three calendar months from the preceding accouchement), Mrs. T. was delivered of another male infant, completely formed, and apparently in good health. When about three months old, he died from measles. From November 1807, to February 1808, Mrs. T. had not left Palermo, except on short excursions in her own carriage; and her husband had been constantly with her since the year 1805. He communicated this narrative to Dr. Maton, with a certificate pledging himself to its truth. It was ascertained, from careful inquiries made by Dr. Paris, of Dr. Maton, that both children were *perfect*.

Ventura,¹ in the early period of his practice, attended the wife of a man named Simon Cabezas. She was delivered by a midwife of a child of fair size, which lived seven and a half months; and before the end of five months, he attended her in her confinement with another, which was larger than the first, and lived four years.

MM. Laudun and Bret² relate the case of a woman, who, on the 23rd brumaire, an. vi (13th November, 1796), was delivered of a full-grown child. The lochia were suppressed on the fourth day, and there was no secretion of milk. In six weeks, she perceived the movements of an infant: and on the 28th germinal (11th April, 1797), she was delivered of a child at full term. This time the lochia and milk were abundant.

Dr. Mæbus,³ of Dieburg, relates the following case. A woman, on the 16th October, 1833, was delivered of a living female child of full size; and on the 18th November, of another, also at full term.

Haller⁴ refers to the following cases, as examples of Superfoetation.

Birth of a living and healthy fœtus twenty days after another.

Children born successively on 13th June, 24th June, and 10th July.

Interval of forty days between the births.

A smaller child born fifty days after the former one.

Births at the seventh and ninth months (Aristotle).

Births of two children at full term, with an interval of two months.

The movements of one fœtus were perceived two months after those of the other.

Child born three months after the former one.

Children born on the 7th April and 27th July (Fischer); also in September and December; also in April and August; and in April and September.

Birth of a male child, followed on the thirty-fifth day by that of a girl; and this, after a hundred and forty days, by another boy.

A weak fœtus, born six months after the first.

Mature and healthy children, born on the 31st July and 9th February (Bartholin).

¹ VENTURA. Op. cit. p. 92.

² Recueil Periodique de la Soc. de Médecine, tom. ii, 1797, p. 384.

³ HENKE'S Zeitschrift, Bd. 31, Heft 2.

⁴ HALLER. Op. cit.

The *Gentleman's Magazine* for March, 1814, relates a case in which a woman was delivered of two boys, and in six weeks of two others.

The cases which I have just enumerated are those which have given rise to the greatest difference of opinion, among those who have treated of the subject of Superfætation. Some, among whom is Haller, are disposed to consider them as instances, in which a second (or even a third) conception took place during the presence of an embryo in the process of development in the uterus; while others, rejecting this idea, have expressed their opinion that they are merely twins, of which one has been retarded in its growth, and has remained in the uterus until fully developed. And, when we attempt to settle the question by analyzing those cases which have been most fully reported, and which consequently seem to afford us some data for conclusions, we find it impossible to determine which of these opinions is right; for, while some cases may be considered as affording proof of the occurrence of Superfætation, others can with equal, or perhaps greater propriety, be classed among twin pregnancies. The imperfect manner in which many of the cases are recorded do not enable us to state, with any degree of certainty, under which of these heads they may be placed; but there are, I think, sufficient data in some of them to warrant the conclusion that Superfætation is possible in certain instances.

The opponents of the doctrine of Superfætation assert that it is impossible, because the uterine orifices are closed soon after conception, and because menstruation is suspended during pregnancy; and that, therefore, a second impregnation cannot take place. Dr. Allen Thomson, after admitting that sexual intercourse for a few days after conception (most probably previous to the closure of the uterus,) may produce a Superfætation, makes the following remarks. "Although it may be, that the mechanical obstruction of the decidua opposes an obstacle to the passage of semen upwards, or to the descent of a new ovum into the uterus, there is obviously another cause why Superfætation should not occur; we mean that fundamental change in the constitution which is induced by pregnancy, similar to that which continues in the majority of women during lactation. But for such a constitutional change, we conceive continual derangement of the function of utero-gestation would attend that process, in consequence of the recurrence of some of the more general symptoms of conception, even though the lodgment of a new ovum in the cavity of the uterus were possible."¹ Dr. David D. Davis² believes that the influence of gestation in causing a suspension of the catamenial secretion is universal and without exception: and, notwithstanding the statements of menstruation having occurred during pregnancy, he is decidedly of opinion, "*that genuine menstruation has really never existed during gestation.*" He grounds his assertion further on the argument, that the orifice of the womb is hermetically sealed during gestation; in consequence of which, no description of fluid, whether the produce of the menstrual function or any other, can by any possibility escape out of its cavity without the previous disturbance of the strongly adhesive

¹ Cyclopædia of Anatomy and Physiology, vol. ii, art. GENERATION.

² Principles and Practice of Obstetric Medicine, vol. i, pp. 252-3. 1836.

plug, by means of which nature has closed up the orifice. He allows that sanguineous discharges sometimes proceed, during pregnancy, from the genital passages; but asserts that, in a great number of cases, they are followed by the expulsion of the ovum. And, as to the few recorded cases of periodical menstruation during gestation, he thinks that they are to be referred to extra-uterine sources, and that periodicity is no proof of their being menstrual.

The occasional occurrence of menstruation during at least the early months of pregnancy, has long been admitted; and there are a sufficient number of cases recorded by competent observers to place the matter beyond a doubt. Mauriceau mentions a case, in which a woman menstruated periodically with each child, up to the sixth month. Brierre de Boismont¹ states, that he has seen five cases, in which the menses continued to flow for two, three, and four months; and three, in which they continued during the whole period of uterogestation. Dr. Rigby² considers that "the regular appearance of the menses for the first few months of pregnancy is of such frequent occurrence, as to place the matter beyond all doubt"; and Dr. Montgomery³ is perfectly convinced that it sometimes occurs.

Admitting then, as I think we must do, the possibility of menstruation taking place during pregnancy, we may inquire under what circumstances it occurs? Many of those who admit its possibility say, with Dr. Davis, (who denies that the fluid is menstrual,) that the discharge may be furnished by the vessels of the exterior of the cervix and vagina, which assume a periodical catamenial function. It is highly probable that many cases are of this kind; and that if the menses flow during the later months of pregnancy they may arise from the vaginal portion of the uterus; but I think it scarcely necessary to ascribe the flow to an extra-uterine source in all cases. The uterus may furnish it from its inner surface. In cases of double uterus, to which I shall presently refer, the unoccupied cornu may furnish the secretion. As for the plug at the os uteri opposing its exit, I do not think this is at all a difficulty; for the secretion may remove an imperfectly formed plug. Ventura⁴ makes some remarks, which, no doubt, contain a deal of truth. "Superfœtation can occur in women of very sanguineous temperament, whose menses are very abundant before conception. In these, the fœtus, in the first months of pregnancy, cannot consume for its nutrition all the blood contained in the uterine vessels Under some circumstances, this viscus is compelled to unload itself of the superfluous blood, which it excretes through the vessels of the neck, and through many of those of the surface, where the placenta is not adherent. Hence the periodical continuance of the menses for three, four, or five months: and, as this cannot take place without a manifest dilatation and relaxation of the os uteri, if at that time cohabitation take place,

¹ BOISMONT (A. Brierre de). *De la Menstruation considérée dans ses Rapports Physiologiques et Pathologiques*, p. 157. Paris: 1842.

² RIGBY (Edward, M.D.). *A System of Midwifery*. 1841.

³ MONTGOMERY (W. F., M.D.). *An Exposition of the Signs and Symptoms of Pregnancy, the Period of Human Gestation, and the Signs of Delivery*. 1837.

⁴ Op. cit.

there is no doubt that the semen may reach one of the Fallopian tubes, and produce a new conception, which is termed Superfœtation."

The periodical occurrence of the catamenial flow during pregnancy, must be considered as a proof of its being connected with ovarian excitement, and consequently with an aptitude for conception. It evidently obeys the same laws as those which usually regulate the appearance of the menstrual secretion. Dr. Meigs,¹ of Philadelphia, in his valuable work on Obstetrics, published during the present year, thinks that periodical ovarian excitement may go on throughout pregnancy. He says: "The conception does not necessarily put a stop to the periodical development of ovarian ova, nor to their maturation and fall. But a woman who menstruates because of her oviposit, will tend to menstruate at regular periods, though she have conceived in the womb. Some women have this tendency so strongly, that they do actually menstruate during the earlier months of their gestation. The above may serve as an explanation of the very common opinion, that a woman is liable to abortion at periods coinciding with the menstrual effort." (p. 213.) Dr Tyler Smith² recognizes menstruation as occurring during pregnancy, especially in the early months. He observes, that "almost all women can perceive the menstrual periods as they pass through utero-gestation, particularly at the first three or four periodic dates. Those who have suffered from menorrhagia or dysmenorrhœa, or in whom organic ovarian disease has existed before conception, recognize the menstrual nismus most clearly; and it is precisely in these subjects" (and at these periods) "that abortion is most likely to occur."

It seems evident to me, then, that the alleged absence of the catamenial secretion (and, by implication, of the ovarian nismus), cannot be adduced as a proof against the possibility of Superfœtation taking place during the earlier period of utero-gestation. The first of these cases related by Ventura (p. 1090), and the one immediately following, tend to support the position I have endeavoured to advance—that Superfœtation is possible, if menstruation occur during the early months of pregnancy. The points most worthy of note are, the occurrence of menstruation subsequently to conception, and the size of the smaller child corresponding, or very nearly so, with the last appearance of the catamenia. It may be objected, that these were cases of unequally developed twins; if so, the coincidences were very extraordinary.

It may not even be necessary that obvious menstruation should take place. Some women have become pregnant, though they have never apparently menstruated: and it is very possible, that the *essential* phenomenon of menstruation—ovarian excitement—may be present to such a degree, as to allow the possibility of Superfœtation, provided that no other impediment exist. The following case, related by Dr. Thomas B. Taylor, of Princetown, Mississippi, U.S., is an apt illustration of my meaning. Clarissa, a negress, the property of Mr. A. Knox, aged about 35 years, in May 1848, was delivered of twins; one a mulatto, and the other a negro child. She had been married several years

¹ MEIGS (Charles D., M.D.). Obstetrics, the Science and the Art. Philadelphia: 1849.

² SMITH (W. Tyler, M.D. Lond.). Parturition, and the Principles and Practice of Obstetrics. London: 1849.

to a negro man on the plantation, of delicate constitution, and had had several children by him. Her menstrual discharge had occurred for several months previous to her pregnancy, at the full of the moon. She felt herself pregnant by her customary signs, about the middle of the month; and, to confirm her suspicions, at the next period the menses did not appear. About three weeks from the time she first felt that she had conceived, and one week after her menses had failed to appear at the regular period, she had sexual intercourse *once* with a white man. At birth, the mulatto child bore marks of being at least three weeks younger than the negro; thus sustaining the woman in her supposition, as to the time between the two conceptions.¹

This case appears to me a most conclusive one: for there can be no doubt that the second child (the mulatto) was the product of a second conception, at the first menstrual period during gestation, when, though there was no apparent flow of the catamenia, the aptitude for conception still remained in force. The different colours of the children, and indeed the whole circumstances of the case, render it highly improbable that it was a mere case of unequal twins.

A somewhat similar case was mentioned by M. Chevroin,² at the meeting of the Academy of Medicine of Paris, September 20, 1842. A negress, at Guadeloupe, was delivered of a black child, and, after an interval of three months, of a mulatto.

Sexual excitement, during pregnancy, is a recognized cause of abortion. It probably operates, in many cases, by means of the increased ovarian excitement which it produces, especially if it take place at the catamenial periods. But it does not follow that, in every case, abortion is the necessary consequence: and I think that, whenever there is a physical or physiological possibility of Superfoetation, several varieties of circumstances may be induced.

1. Abortion may occur, either through irritation of the uterine or vaginal nerves, or through increased ovarian excitement. If a second ovum have been impregnated, and have descended into the uterus, it may be also expelled.

2. The abortion may take place before the descent of a second ovum, which thus is removed from danger, and is capable of being developed forthwith in the uterus.

3. The excitement may fail to act on the uterus so as to call its expulsive powers into action, while a second impregnated ovum may descend into the womb, and there be developed. This would form a true case of Superfoetation; and I think that, although of rare occurrence, it is far from being impossible.

These remarks are founded on the supposition, that there may be no mechanical impediment, from closure of the os uteri or of the orifices of the Fallopian tubes, to the fecundation of a second ovule. But probably, in many cases, the mechanical impediments at the uterine orifices render a second conception in the human subject impossible. This is evidently a wise provision of nature against the continued fertilization of ova, and their descent into the uterus, during gestation.

¹ New Orleans Journal, November 1848; and American Journal of the Medical Sciences, April 1849, p. 549.

² Gazette Médicale de Paris, 1842, p. 622.

If it were not so, we might expect Superfætation to occur much more frequently,—provided that abortion of the already existing embryo were not induced.

Another explanation of the cases of alleged Superfætation, and one which is probably correct in a large number of instances, is that a twin has been arrested in its growth. It may then either be expelled at the same time with the mature fœtus, or it may be retained until it have arrived at its full development. In the latter case, why are not both expelled together? To answer this question, we must be acquainted with the exciting cause of parturition. If this be the return of the ovarian nîsus, at a catamenial period, then why does not the expulsive power of the uterus produce the abortion of the imperfect embryo, as well as the normal delivery of that which has arrived at maturity? If, on the other hand, we recognize the condition of the fœtus and placenta as an accessory circumstance, having an influence on parturition, then there is some reason for supposing that this cause operates in the retention of the smaller child, especially if it have arrived at a period of development in which the danger of abortion is diminished. This reasoning will equally apply to the cases, in which there is every probability that Superfætation has taken place.¹

It is difficult to form a diagnosis between cases of retained twin and of true Superfætation, though several authors have attempted to do so. Gravel states, that they may be distinguished:—1. By twins being of equal size at first, but afterwards becoming unequal, as one grows more than the other. 2. By the movements of the smaller twin being much more obscure than those of the other, while, in Superfætation, the movements of the second fœtus are strong, and gradually increase. Foderé lays down the following distinctive marks:—1. The second child, in Superfæations, is usually larger and more healthy than the first. 2. Twins have generally a single placenta, while the product of Superfætation has its placenta distinct from that of the other child. 3. There is a great interval, in the cases recorded, between the births of the children. He denies that a *twin* may be retained more than a week or two. I do not think that the data given by Foderé can be depended on as means of diagnosis. It would be very difficult to prove that the second twin may not attain greater size and strength than the first, if it be retained for a sufficient length of time, in its favourable circumstances of increased space, and possibly of increased supply of aliment. Foderé lays great stress on the cases of Bigaud and Franquet, in which the lochia and milk were suppressed until after the birth of the second child, as affording proofs of Superfætation. But it is not unlikely, that this retention was an expedient of nature, to preserve a sufficient quantity of nutritive matter for the retained fœtus, whether a twin or not: and if the aliment be so carefully retained, we can see why the second twin, as well as the product of Superfætation, may be larger and stronger than the first. As to the placenta; twins have not always a single placenta, but separate ones, attached to different

¹ Gravel has a very curious idea as to the cause of the retention of the second fœtus. "Cur vis uteri contractilis non ultimam operi manum admovet? Scilicet tum demum infanticidium abhorrens anima, relicto in pineali glandulâ throno, ad uterum descendit, et embryonem prehensâ manu detinet, dum fœtum perfectum calcibus foras extruderit."

portions of the uterus. If there be a single placental attachment, the probability of the two fetuses being expelled at the same time is increased,—though this is not a necessary consequence.

The cases related by Ventura and by Dr. T. B. Taylor, are instructive in furnishing data for diagnosis. In both of these, the coincidence between the apparent age of the smaller child at birth and the supposed date of its conception, is too striking to be considered as merely accidental. The different colour of the children in the last case is an additional proof. But we have not always the data furnished with such accuracy; and we can only conclude, with Orfila,¹ “that the physician consulted in such cases, should admit the possibility of Superfoetation, but state, at the same time, that it is very difficult to distinguish this from abortion, or from twin pregnancy”.

III. There is another class of cases, which prove the possibility of a second conception taking place at an early period: viz., those in which two children have been born of different colours. One of these I have already related at page 1096, as it had a bearing on the subject there under consideration.

A case is related by Buffon,² of a woman at Charlestown, in South Carolina, who was delivered, in 1714, of twins, within a very short time of each other. One was found to be black, and the other white. This variety of colours led to an investigation; and the female confessed, that, on a particular day, immediately after her husband had left her, a negro entered the room, and, by threatening to murder her if she did not consent, had connexion with her.

Dr. Moseley³ mentions the following as occurring on Shortwood Estate, in the island of Jamaica. A negro woman brought forth two children at a birth, both of a size; one of which was a negro, and the other a mulatto. On being interrogated as to the occasion of their dissimilitude, she said, that a white man belonging to the estate came to her one morning, and she suffered his embraces almost immediately after her black husband had quitted her.

Dr. Turton⁴ attended a negress, named Maria Johnson, who was delivered of a black child at the eighth month, which lived two hours. After some hours, a four months' white fetus was expelled. This, though a possible, is but a doubtful case of Superfoetation; for the smaller fetus may have been a negro, which had not yet acquired colour.

Mr. Richard Dick⁵ related the following case. A negro woman, an apprentice on Orange Park Plantation, in the parish of St. David, Jamaica, was, on the 12th of December 1834, delivered of two male children, one of which was a fair mulatto, the other a negro, both of which lived. The woman denied having had any intimacy with a white man.

¹ ORFILA, *Traité de Médecine Légale*. 4me Edition. Paris: 1848.

² Quoted by Foderé, vol. i, p. 462; by Beck, p. 160; etc. This case seems to be inaccurately reported; for one of the children should have been a mulatto.

³ Moseley on Tropical Diseases, p. 111. Quoted by Beck.

⁴ London Review, No. 16, 20th October, 1823.

⁵ American Journal of the Medical Sciences, vol. xvii, 1835, p. 188, quoted from *Jamaica Physical Journal*.

Dr. Holcombe,¹ of West Granville, Massachusetts, was acquainted with female twins, eight or ten years' old. One was a well marked negro, the other a mulatto, presenting less than usual of the negro appearance. The mother, a negress, confessed having had intercourse with a white man. The black child had a strong family resemblance to the black father. All the other children of the family were very black.

A married woman, aged 22, was brought to bed of twins in the Lying-in Hospital at Berlin,² on the 25th of January, 1832. The children were both girls, and died in two hours after birth, which took place before the end of the seventh month. One child was white, the other evidently a half-caste, as was indicated by the shape of the head, and the leaden tinge of the face, hands, and feet, which in colour resembled those who have been tinged by taking nitrate of silver in large doses. The same difference of colour was strikingly exemplified in the umbilical chords of the infants, but not in the membranes or placenta. On enquiry, it appeared that she was in the habit of intimacy with a negro, shortly after, or at the time, when she had conceived by her husband.

Dr. Dewees³ has related, that a servant in Montgomery county was delivered of a black and a white child at one birth. He states also, that on the report of the pregnancy, both a black and a white man disappeared from the neighbourhood.

Dr. Elliotson⁴ relates, on the authority of Mr. Blackaller, of Weybridge, the following case. A white woman, of very loose character, left her husband, and some time after, returned pregnant to the parish, and was delivered in the workhouse of twins, one of which was of a very dark colour; the hair quite black, with the woolly appearance usual to negroes, with flat nose and thick lips. The other had all the appearances common to white children.

Dr. Dunglison relates,⁵ that one of his pupils, Mr. N. J. Huston, of Virginia, communicated to him the particulars of the case of a female, who was delivered, in March 1827, of a negro child and a mulatto on the same night.

Several similar cases are referred to in the foot-note at page 160 of Beck's *Medical Jurisprudence*; and Dr. Dunglison has observed, that, "where negro slavery exists, such cases are sufficiently numerous".

A very remarkable case is related by the Rev. Dr. Walsh,⁶ which was communicated to him by the Sargenté of San Jose gold district (Brazil). A Creole woman had three children at a birth, of three different colours, white, brown, and black, with all the features of the respective classes.

These cases prove the possibility of a second conception taking place soon after the first. In several of them, the confessions of the women serve to remove all doubt. The ovum first impregnated had probably not descended, nor had the uterus been placed in a condition to oppose

¹ Boston Medical and Surgical Journal, vol. xiii, p. 64.

² Dublin Journal of Medical and Chemical Science, quoted in *London Medical and Surgical Journal* for 1834.

³ Philadelphia Medical Museum, vol. i.

⁴ Notes to Blumenbach's Physiology, p. 485.

⁵ Dunglison's Physiology, vol. ii, p. 324.

⁶ Walsh (Rev. Dr.). Notices of Brazil, vol. ii, p. 90.

an obstacle to further fertilization. There may be something, too, in the difference of races. In some animals, the impregnation of the female, near the same time, by males of different breeds, has produced offspring resembling each of the male parents. It is very difficult to distinguish such cases of consecutive conception from twins. If a woman conceived of children, after cohabiting successively with two men of her own colour, the offspring would doubtless be considered twins, in the strictest sense of the term, unless a strongly-marked paternal resemblance gave room for a different opinion.

IV. In the foregoing cases, I have reasoned as if the uterus were single. But the presence of a Double Uterus has been much insisted on as explanatory of the doctrine of Superfetation; and, it is not improbable, that in several of the cases to which I have referred, this peculiarity of structure existed; though, in some, as in that mentioned by Professor Eisenmann, the cavity of the organ was single. This malformation seems to be of not unfrequent occurrence; and several writers have recorded a large number of cases in which it existed. Dr. Cassan, of Paris, in the year 1826, published a thesis entitled *Recherches sur les Cas d'Uterus Double et de Superfétation*. Voigtel, in his *Handbuch der Pathologischen Anatomie*, gives a copious list of cases; and Madame Boivin, and Professor Rokitansky, have observed this structure in a pretty large number of subjects. It has, like most other malformations of organs, its analogue in the lower classes of mammalia. Dr. Rigby¹ gives a good summary of the different forms of the uterus in animals. "The nearest to the tubular uterus, and where the transition from the oviduct in birds, etc., to the uterus in mammalia, is least distinctly marked, is the *uterus duplex*. Although the uterus is double, there is but one vagina into which the two ora uteri open; there are as yet no traces of a cervix, each os uteri merely forming a simple opening, at the lower end of what is little more than a cylindrical canal. We do not find that thickening at the lower extremity of the uterus which distinguishes the cervix in the higher mammalia. This species of uterus is found among a large portion of the rodentia, and is also occasionally met with as an abnormal formation in the human subject. The next grade of uterine development appears under the form of the *uterus bicollis*. The double os uteri here ceases to exist, and the division begins a little higher up, so that the two cavities of the uterus communicate for a short space; the ova, however, do not reach the common cavity, but remain each in its separate cornu. In this form of uterus, the os uteri is not only single, but the lower portion is thickened, although it has not yet formed a distinct neck or cervix; it is met with among some of the rodentia, and also certain carnivora.

"In the *uterus bicorporeus*, the union of the cornua is higher up, so that the lower portion is single, while the upper part alone is double, consisting of two strongly curved cornua. This formation is peculiar to ruminating animals. If two ova be present they are separate from each other, each being contained in its own distinct body or cornu, but a portion of the membranes extends along the common cervix, from one body to the other.

¹ Op. cit. p. 15, 16.

"A still higher grade is the *uterus bifundalis*, where the fundus alone is double, the cornu being formed only by this portion. This formation is observed in the horse, ass, etc.: the common cavity is here the receptacle of the ovum, so that in the impregnated state, the cornua appear only as appendices, into which a portion of the membranes extend.

"In the *uterus biangularis*, the double formation has nearly disappeared, except at the fundus, where the uterus imperceptibly passes into the tubes: this is the case among the edentata, and some of the monkey tribes.

"The highest grade is the *uterus simplex*: every trace here of the double form is lost; the fundus no longer forms an acute angle, where it bifurcates into two cornua; but is convex. We now for the first time see the divisions of the uterus into body and cervix distinctly marked.

"The human uterus presents a similar variety of forms, as it gradually rises in the scale of development during the different periods of utero-gestation. It is at first divided into two cornua, and usually continues so to the end of the third month, or even later; the younger the embryo the longer are the cornua, and the more acute the angle which they form; but even after this angle has disappeared, the cornua continue for some time longer."

The term *double uterus* has been objected to by Drs. Robert Lee and D. D. Davis, on account of there being but one ovary and one Fallopian tube to each division. It seems, however, convenient to retain the term, as comprehending all the cases in which the uterus consists of two distinct cavities. Dr. Davis thus classifies them after Voigtel:¹—"Either there is one uterus, with a single uterine aperture and one vagina, the space allotted to the uterine cavity being divided into two parts by a distinct membranous septum; or the body of the womb is found to consist of two parts perfectly distinct and separate from each other, but uniting in a common aperture into a single vagina; or there are two uteri distinct and separate throughout, each having its distinct opening, and each communicating with its appropriate vagina".

In those animals in which a double uterus exists, Superfœtation is stated to be of common occurrence, *e. g.* in the hare and other rodentia. It is somewhat remarkable, that although Superfœtation in the human female has long been supposed to be closely connected with this peculiarity of structure, but few cases have been recorded in which these were actually discovered to co-exist. We may then inquire, whether there are any inherent circumstances in the existence of a double uterus, which show the probability of its becoming the seat of a second conception.

Does menstruation occur in the empty cornu? Cassan says, that this idea is opposed to his observations. Dr. Oldham,² in his recent report on a case of labour complicated with a double state of the internal sexual organs, says, in speaking of double uterus, "However the sexual passages may appear to be double, and the one side physically independent of the other, yet they appear to me to be as one organ in

Op. cit. p. 514.

² Guy's Hospital Reports, October 1849.

their physiological relations; and the best proof of this is to be found in the manner in which menstruation is performed. In this there has been nothing erratic, and we may infer that both wombs menstruate at the same time; and what, perhaps, is still more to the purpose, when one womb has contained a fœtus, the other has ceased to menstruate, and with this suspension it loses, I believe, its aptitude for impregnation". Our knowledge of the physiological relations of symmetrical organs, would naturally lead us to infer the simultaneous performance of the catamenial function by both cornua of the divided uterus. But, as I have already stated, menstruation is sometimes known to occur during pregnancy, in common cases; and the ovarian stimulus is perhaps often more active than we are aware of, though the presence of a fœtus has sufficient influence to prevent any obvious indication of it. Now it is very likely, that the empty cornu may, in cases where the ovarian excitement is sufficiently powerful, continue menstruating as long as the tendency remains in the other; and if so, then there is a possibility of the occurrence of Superfœtation. In a case related by Dionis, in 1681, of a double uterus, in one cornu of which a fœtus had been formed, menstruation was stated to have occurred during the first four or five months of pregnancy.

Dr. Robert Lee¹ has founded an objection to the possibility of the occurrence of menstruation or Superfœtation in a double uterus, on the formation of a decidual membrane in the empty cornu. In a case described by him, the body of the uterus was cleft from fundus to cervix, so as to form two lateral halves. The right cornu had contained a fœtus, and resembled the uterus a week after delivery; (the woman had died eight days after her confinement). The right ovary was much larger than the left; and the latter presented no corpus luteum. The left uterine cornu was of the size of an unimpregnated uterus. Its parietes were very soft and vascular; and the inner surface was everywhere coated with a delicate and beautifully-formed deciduous membrane. At the opening of the cornu into the cervix, the deciduous membrane formed a short sac; but it presented a smooth circular opening at the uterine orifice of the Fallopian tube. The fibres of this membrane, as they approached the opening of the tube, ran in a converging direction, and passed into the opening, leaving it completely pervious. The distance to which the fibres extended into the tube was not ascertained, nor was it positively determined whether the whole extent of the canal were open. There are some circumstances, however, which seem to me to throw a doubt over the fact of the decidual membrane in the empty uterus having been formed during pregnancy: these are, its delicate structure, its not having been discovered till eight days after delivery, and the woman having died from peritonitis. In other records of cases of pregnancy occurring in double uterus, I have not found it mentioned, whether or not the empty cornu was lined by any membrane. It may be formed in some cases, and may have existed during pregnancy in Dr. Lee's case, as supposed by him. But even then, Superfœtation must be admitted as possible, if we believe in its

occasional occurrence in a single uterus, while containing a fœtus and decidua. There is not, then, I think, any physiological reason why Superfætation should not take place in a double uterus; and an examination of some cases which have been recorded, will serve to confirm the view which I have taken of the possibility of the occurrence. I would remark, however, that it is not to be supposed, that an aptitude for secondary conception exists in all women who have a double uterus; it will be in those only, in whom the ovarian nus is sufficiently active, either with or without the actual occurrence of the menstrual flow.

Marquet¹ relates, that at the post-examination of a woman who died of phthisis, a double uterus was found, resembling two inverted pears, united by their neck, terminating in a common orifice. She had had fourteen abortions, always accompanied with uterine hæmorrhage and other alarming symptoms. On one occasion, she brought forth twins, with a single placenta, at four and a half months: and in a month after, was delivered of a fœtus, apparently of six weeks' growth.

Cassan² has recorded the following case, which occurred to Madame Boivin. A woman was delivered, on the 13th of March, 1810, of a female infant weighing four pounds. The abdomen still remaining bulky, Madame Boivin introduced her hand, but could find nothing in the uterus, which was already much contracted. But the examination, and the occurrence of fœtal movements during the next two months, led Madame Boivin to infer, that there must be either an extra-uterine pregnancy, or a Superfætation in a bilobed uterus. On the 12th of May, another female infant was born, weighing about three pounds, feeble, and scarcely able to breathe. The woman said that she had had no connexion with her husband for some time, except on the 15th and 20th of July, 1809, and on the 16th of September.

Dr. Dugès³ has related a case, communicated by a pupil of the Maternité at Paris. A woman, in her seventh pregnancy, aborted in the fifth month of a dead fœtus, eight inches and a half long, which was followed by an embryo, about three inches and a half long, apparently of the third month, shewing some signs of life. The two placenta were discharged separately: they were of different sizes. On careful examination with the hand, the vagina and cervix uteri were found to be natural; but beyond the exterior opening of the latter, there were two orifices, each leading to a distinct uterus.

Dr. Geiss⁴ has recorded a case, which, although deficient in some details, must be considered as a probable instance of Superfætation. During his attendance on a lying-in patient, he observed, that the pains were entirely limited to the right side of the uterus, and that this side was elevated as high as the thorax, the left only extending to the umbilical region. On examination, the shoulder of a fœtus enclosed in its membranes, was distinctly felt. Immediately after the birth of this child, the right side of the abdomen diminished in size, while the left side remained in the same condition as before. In about an hour,

¹ MARQUET. *Traité pratique de l'hydropisie et de la jaunisse.*

² CASSAN. *Récherches sur les Cas d'Uterus double et de Superfætation.* Paris: 1826.

³ *Journal des Progrès*, vol. xiii, from *Ephemer. de Montpellier.* *London Medical and Surgical Journal*, 1830, p. 172.

⁴ *Rust's Magazine*, quoted in *London Medical and Surgical Journal*, vol. ii, 1829, p. 259.

labour pains returned; and Dr. Geiss discovered, on examination, beyond the orifice of the uterus, a membrane distended with fluid, projecting through an annular aperture in the left side of the uterus. The umbilical cord of the infant born was traced to the upper part of a distinct cavity. On further examination, the abdomen of a child was distinctly felt at the opening. Turning was found necessary, as with the former child: it was accomplished with some difficulty. Dr. Geiss introduced his hand, and convinced himself that the organ was divided. The placenta in the right half of the uterus was first thrown off, and this part of the organ contracted vigorously; but, in the detachment of the second placenta, the contractions were feeble, and the woman lost much blood. She ultimately recovered. Two years before, she had been delivered of a single infant, after a very difficult labour.

Billengren¹ relates the case of a woman, forty years old, who, after having uterine hæmorrhage for eight days, was delivered of a foetus with its membranes, apparently not more three months old. A second foetus, of above seven months, was in the evening extracted by means of the forceps, but soon died. The uterus was divided by a septum into two cavities, each of which contained a placenta.

A very remarkable case, which places the connexion of Superfoetation with double uterus beyond a doubt, has been lately recorded by Dr. Generali.² Gaetana Bovatti, of Modena, had had six pregnancies. Her labours, though successful, had always been difficult, requiring the use of instruments. In 1816, she was pregnant for the seventh time; and there was remarkable, along the median line of the uterus, a well-marked furrow, which caused a double pregnancy to be suspected. On the 15th of February, 1817, she was delivered of a living male infant, apparently at the full period. The placenta was expelled in the natural manner: there was no lochial discharge. The abdomen was reduced in size on only one side, and the movements of a foetus were distinctly perceived on the other. On the 14th of March, she was again seized with labour pains, and was delivered of a second male infant, of equal development with the first. The first of these lived forty-five days, the latter fifty-two days. In 1822, she was delivered, for the last time, of a female child, who still lives. This case created a great sensation; and Professor Bignardi regarded it as an instance of Superfoetation, due to an abnormal conformation of the uterus. In September 1847, the woman Gaetana Bovatti died of apoplexy. Dr. Generali examined the uterus, and found that it possessed the structure, the existence of which had been suspected by Professor Bignardi. The neck of the uterus was of the usual form; but the body of the organ was divided into two parts, each of which was furnished with a Fallopian tube. This interesting specimen of teratology has been deposited in the anatomical museum at Modena.

These cases may possibly be doubted to be instances of Superfoetation. It may be said they were cases of arrested twins; but a careful examination of the phenomena which they present, will, I think, serve

¹ Svenska Läkarsällskapet's Nya Handlingar, Band ii. Quoted in *Neue Zeitschrift für Geburtskunde*, 1846, p. 147.

² *Bulletino delle scienze Mediche di Torino*.

to establish the probability of the occurrence. We may then conclude, that Superfœtation may take place, and has done so, in cases of double uterus. But, even including many of the cases in which no double uterus is mentioned, as if the organs were in this condition, the proportion of cases of double uterus with Superfœtation is but small, compared with those in which it has been accompanied with single pregnancy. It may be that, in some cases, as I have already observed, the ovarian excitement, or menstruation, may not be present; or, in others, there may be a mechanical obstacle to the fertilization of the second ovum. But there is another circumstance, which may operate in reducing the frequency of Superfœtations in double uteri; and that is, the abortion of the fœtus already present, where an attempt is made at impregnation. In Marquet's case, the woman had aborted fourteen times; and, in other cases in which a double uterus has been found, there has been abortion. This may have arisen in part from circumstances connected with the abnormal structure itself, which have caused the presence of double uterus to be looked on as a source of danger. Orfila¹ particularly notices the danger, and calls attention to the fact, that a large number of the women who have conceived in one of the cavities of a double uterus have died at, or soon after delivery. In other cases, the labours have been difficult. I do not, however, purpose to enter here on a discussion of the dangers attending on a double uterus in the human subject.

V. The next class of cases, to which I shall refer, comprehends those in which pregnancy has taken place during extra-uterine gestation. The following are instances.

Dr. Cliët, of Lyons,² found in the body of a woman, who had had several children, and had died suddenly, a male fœtus of about five months, lying in the right iliac fossa. The uterus contained another, of the male sex, of about three months.

Dr. Montgomery³ relates a case, in which a woman was the subject of an extra-uterine pregnancy for three years, during which time she bore three children. The extra-uterine fœtus was at length expelled through the walls of the abdomen, near the umbilical.

Several similar cases have been recorded, which prove the possibility of the occurrence: and seem to show that if the ovarian nîsus be suspended for the usual period of utero-gestation in such cases, it is resumed when the usual time of delivery returns. As to the formation of a decidual membrane, Dr. Robert Lee has not found it; Chailly says, that it is present: and Dr. F. H. Ramsbotham thinks that it is formed, but is expelled. In any case, it cannot be an obstacle to conception.

The difficulties attending on the investigation of the subject of Superfœtation, and the imperfect records which have been given of many of the cases, prevent us from arriving at any fixed and ultimate conclusions. I would, therefore, not be understood as attempting to determine all the disputed points connected with it, especially the diagnosis between the true cases of Superfœtation, and those in which a twin

¹ ORFILA. *Op. cit.*

² *Nouveau Journ. de Médecine.* Dec. 1818.

³ *Op. cit.*

has been arrested in its growth. An extended series of observations is required; and, as the cases are but rare, it will probably be a considerable time before all the facts connected with Superfoetation are known. A careful reflection on the cases which have been recorded, leaves in my mind no doubt of its having actually occurred, both where the uterus has been of the usual form, and where it has been double, or bilobed. It may take place during the first days of pregnancy, before the formation of the uterine plug; and also subsequently, within a limited period, if this plug have been removed by any cause, provided that the necessary ovarian nisis be present.

Essex House, Putney, November 1849.

EXPERIMENTAL AND PRACTICAL OBSERVATIONS ON THE USE OF CHLOROFORM IN MIDWIFERY;

AND ON THE INFLUENCE EXERTED BY THIS ANÆSTHETIC UPON THE
MOTOR POWERS CONCERNED IN PARTURITION.

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I AM not aware that any experiments have been performed, with a view to decide upon the influence of Chloroform on uterine muscular action. The following is a contribution to this interesting subject, and may, perhaps, tend to throw some light upon the physiological relations of Chloroform to Obstetric practice. Chloroform promises to be a great boon to the experimentalist, since it renders the most formidable vivisection as painless as dissection after death. It must, however, be borne in mind, that from the intense action of this agent upon the nervous system, it is extremely liable to complicate any nervi-motor experiments in which it may be employed. But this objection does not apply to experiments upon the action of Chloroform itself upon the nervous centres. In experiments of this latter kind, we can witness the action of Chloroform without the disturbing influence of pain; in all other experiments, we certainly lose the interference of pain and all its results, if we employ Chloroform, but we inevitably have a certain amount of derangement of the nervous functions from the anæsthetic. My present object is not so much to deal with the question of the propriety or impropriety of Chloroform in midwifery, as to inquire, how Chloroform acts towards the motor powers concerned in natural parturition, and in the accidents of the parturient and puerperal state dependent upon an abnormal condition of those powers? I would wish to consider it impartially, not as a poison, or as an agent with power to allay the agonies of surgery and obstetrics; but as a medicine capable of modifying the motor powers of the uterus, upon the integrity of which safe and natural parturition depends.

Observation 1. A guinea-pig being placed under the influence of Chloroform, the effects were carefully observed in the uninjured animal. At first, the breathing became hurried and increased in vigour, and the limbs, particularly the posterior extremities, were convulsed. When

the limbs were pinched, they were smartly retracted by reflex action. It was evident that, as a first effect, the function of the spinal marrow was *exalted*. This appears to agree very well with the opinions of numerous observers, to the effect that when parturient women are lapsing into, and recovering from, the anæsthesia of Chloroform, the pains of labour are somewhat increased in power.

Observation 2. After a few minutes, the respiratory movements diminished in force and frequency, becoming chiefly diaphragmatic. The limbs remained stiff, even when pinched or pricked, so that neither *sensation* nor *reflex* motor action remained. Everything now indicated that the energy of the spinal function was *diminished*. When the Chloroform was pushed to a further extent, the sphincters of the rectum and bladder were relaxed, so as to allow of the escape of their contents, and all the muscles of the body appeared loose and flaccid. This appears to agree with what is observed in the obstetric use of Chloroform,—namely, that the muscles of the outlet of the pelvis, and the vagina, are all relaxed (though the relaxation may not reach to the extent of allowing the urine and fæces to escape), the expulsion of the fœtus being thus considerably facilitated.

Observation 3. The spinal marrow being divided in the dorsal region, while the animal was in deep anæsthesia, and the abdomen laid open, a stilet was passed down the spinal canal, so as to destroy the lower portion of the spinal marrow. In ordinary circumstances, violent movements of the limbs attend the laceration of the spinal cord, but here, no movements whatever occurred. Thus the *direct* as well as the *reflex* motor actions were annihilated.

Observation 4. It was observed, that notwithstanding the destruction of the spinal marrow, the uterus, and the rectum and colon, were affected with vigorous peristaltic action, under the stimulus of the air, pricking with a needle, caustic, potash, etc. The guinea-pig is remarkably well adapted for observing the uterine peristaltic action; as even in the unimpregnated state, the cornu and body of the uterus are abundantly muscular, and endowed with peristaltic action.¹

Observation 5. When the Chloroform had been continued sufficiently long to annihilate all the other reflex actions, imperfect acts of respiration still continued. The thorax and the neck being laid open, the *nares*, the *larynx*, and the *diaphragm*, were seen to move simultaneously from time to time, as in inspiration. These organs, associated together by the *facial*, *laryngeal*, and *phrenic* nerves, continued in action some time after all the external muscles of respiration had ceased to move. These three organs were, in fact, the *ultima movens* of respiration; just as respiration is itself the last spinal function which remains active in poison by Chloroform. But for this persistence of respiration, accidents from the use of anæsthetics would doubtless be more frequent than they are at present.

¹ My friend, Mr. Barlow, of the Westminster Hospital, informs me that he has repeated the experiment on the unimpregnated guinea-pig, and found the destruction of the spinal function to be as I have described it, but he failed to observe the peristaltic action of the uterus. I can only account for this by supposing that it had contracted to its full extent before Mr. Barlow directed his attention to it. In my experiments, the uterine peristaltic action was witnessed by Dr. Marshall Hall, and Mr. Henry Smith, of Torrington Square.

Observation 6. After the total loss of the spinal function, whether as tested by the irritation of the excitor nerves, or by the laceration of the spinal centre, the power of the motor nerves still remained. A nerve supplying the abdominal muscles being isolated, and pinched with the forceps, these muscles readily contracted.

Observation 7. A fresh guinea-pig was nearly killed with Chloroform, and a stilet was at once passed through the whole of her spinal marrow, from the cauda to the cranium, but no spinal movement of any kind took place. In other respects, the same phenomena, as those which have been above detailed, took place. The spinal marrow was broken down entirely; still the peristaltic action of the heart, intestines, and uterus had not ceased. The contraction of the heart, intestines, and of the uterus, had each their own peculiarities of action. The heart and the intestines both contracted and dilated, as having to receive, and to transmit onwards, their contents. The uterus only shortens itself, as in an effort to expel its contents through the vagina. The uterus and intestines continued to act, moreover, after the beat of the heart had ceased. Thus there is apparently a certain definite order of dying, in the different organs under the control of the ganglionic system. In the human subject, the uterus evidently contracts so as to deliver its contents, after both heart and intestines have ceased to act; it is the *ultima movens* of the ganglionic system, just as the respiratory organs are of the spinal system.

Observation 8. But, lest it should be said that the spinal function was annihilated in these experiments, not by Chloroform, but by death, or by grave injury to the animal, it was determined to repeat them upon the frog, in which the reflex function remains active for at least a quarter of an hour after death: thus giving abundant scope for the observation of the effects of Chloroform upon the spinal portion of the nervous system. A healthy male frog was selected for experiment.

Observation 9. The frog was placed under a glass inclosing a little bibulous paper, wet with Chloroform. In a very brief space it became quiet, and lost all power of voluntary motion. When the animal became thoroughly under the influence of Chloroform, the most excitor parts of the surface of the body were successively irritated in order to test for reflex action. The conjunctiva, the eyelids, and the anus were pricked with a needle, without inducing closure of the eyelid or retraction of the lower limbs. The toes and the tubercles on the thumb, which, together with the testes, were considerably developed, notwithstanding the early season (October), were pinched without any motor result. Before the application of Chloroform, the tubercles in this frog, and the inner surface of the fore-arm, were found to be intensely excitor. These trials proved that, in the living frog, under Chloroform, the spinal reflex function remains in complete abeyance. Even the respiratory movements ceased entirely. On being removed from the Chloroform vapour, the animal recovered, notwithstanding the cessation of the respiratory movements for a considerable time—its recovery being probably owing to the influence of the skin as a respiratory organ.

Observation 10. A vigorous frog was placed as before, but turned upon its back so as to show the influence of Chloroform upon the heart's action: the impulse of the heart being visible upon the surface of

the body. The influence of the Chloroform was continued until the heart ceased to beat, and the animal was dead. The head was now removed, and a fine needle passed through the medulla oblongata. No movements whatever of the eyelids occurred. In ordinary circumstances in such an experiment, the eyes are drawn in powerfully, at the moment of destroying the upper portion of the spinal marrow.

Observation 11. The needle was now passed down the spinal canal; no movements whatever of the fore-arms took place, nor did any movements occur until the needle had reached the origins of the nerves at the bottom of the canal, when slight movements of the extremities were observed. The laceration of the spinal marrow itself produced no effect. This organ was evidently dead to all excitation, whether applied directly to the spinal marrow itself, or in the form of irritation to excitor nerves.

Observation 12. The nerves, also, were found to have lost their excitability, except at the time when they were irritated, as described, within the spinal canal. A large motor nerve being dissected out, was pinched without causing the slightest movement in the limb to which it was distributed. The galvanic stimulus was then applied to the nerve, with a like negative result. In this respect, the frog differed from the mammiferous animal. In the one, the *vis nervosa* remained active; in the other, it was entirely destroyed.

Observation 13. Another difference was also apparent. In the guinea-pig, the irritability of the muscles remained vigorously active for some time after death. In the frog, the influence of the Chloroform appeared much more intense. The muscles, being stimulated with galvanism, contracted very feebly. Even the muscular irritability, the last motor power which remains under ordinary circumstances, was impaired.

Observation 14. A remarkable phenomenon appeared as the last of the series. The *post-mortem* rigidity, which in the frog is very remarkable, and generally develops itself three or four hours after death, set in within ten minutes from the time when the beat of the heart ceased. The entire animal was extended and perfectly rigid. This fact renders the application of Chloroform, in the case of the frog, a very important agent for studying the nature of this peculiar form of contraction. The entire death of the frog under Chloroform is more rapid than death from the most virulent poisons; the animal dies more quickly than from either hydrocyanic acid or strychnine!

Observation 18. It remains to apply the preceding observations to the use of Chloroform in Obstetrics, and to its influence upon the physiology of parturition. The advocates of Chloroform in midwifery, admit that it exerts some influence upon the motor actions of labour. There are few who maintain, that under its use, anæsthesia is the simple and uncomplicated result,—that it annuls pain without in any degree affecting the motor powers concerned in labour. The advocates of anæsthesia maintain, however, that the perturbation of the natural actions of labour do not produce any ill results.

Observation 19. The opponents of anæsthesia in labour, contend not only that Chloroform affects the motor actions of labour, but that it affects them injuriously, weakening the parturient motor powers to such

an extent as to lead to retention of the placenta, uterine hæmorrhage after and during labour, and inertia of the uterus, to such an extent as to require the forceps or craniotomy, in cases when delivery would otherwise have occurred naturally. Both parties, the Chloroformists and the anti-Chloroformists, appeal to experience. But, in the present state of the matter, the experience on the subject is scarcely voluminous enough, or sufficiently well-authenticated, to justify an authoritative conclusion. Under these circumstances, physiology and physiological experiment can be appealed to, for at least some aid in the solution of the *quæstio vexata*, as well as to account for some of the contradictions which appear in the evidence furnished by experiments. In all such cases, physiology must necessarily be an important court of appeal.

Observation 20. The increased uterine action observed under the administration of Chloroform in natural parturition, would appear to be analogous to the increase of the spinal function on the first inhalation of this agent. A thousand facts in obstetrics testify most irrefragably to the influence of the spinal marrow, and of the reflex function, in parturition. That, under the influence of Chloroform, stimulation of the spinal marrow is observed in the first instance, appears to be proved experimentally. In human labour, the motor influence of Chloroform may be also increased by mental emotion, at the instants of passing into, and escaping from, anæsthesia. In some cases, when the anæsthetic is used in great moderation, its effects, however long continued, may, perhaps, be simply stimulant.

Observation 21. Where the motor pains of labour are rendered less vigorous by Chloroform, we may infer that it is because the function of the spinal marrow, particularly of its lower portions, is enfeebled by Chloroform, as in the experiments in which the reflex actions were so much reduced in power. The uterus and the parturient function are left chiefly to the peristaltic action of the organ.

Observation 22. But we know, that under these circumstances, delivery nevertheless takes place. The pains may appear weaker, but the child is expelled in many cases without any unusual delay, and, in others, after labours of very tardy progress. In many cases where the labour is completed within the common period, labour is said not to be interfered with by Chloroform, though in reality it may be, to a very considerable extent.

Observation 23. It has been mentioned, that in deep chloroformization, the sphincteric orifices of the pelvic viscera are relaxed. In the use of Chloroform in labour, it is universally maintained, that unusual relaxation of the vagina and perinæum is produced. This is naturally explained by the diminished energy of the spinal function. The vagina, the muscles of the perinæum, and of the floor of the pelvis, lose some of that reflex power which affects all muscles, and by virtue of which they resist the passage of the fœtus, until full dilatation has been performed by the gradual advance of ordinary labour. Thus, though in the use of Chloroform in labour, a power of expulsion is taken away, a power of resistance is at the same time withdrawn. In the balance of the two, labour proceeds, and, in some cases, even more rapidly than usual; in others, again, the process is very tardy or impossible. As already observed, Chloroform does not commonly dilate the sphincters when

given in labour, or in operations, to such an extent as to cause the escape of the contents of the bowels and bladder; but it does so when given in excess. In the relaxation of the parturient passages in labour, the dilatation, though partial, is still sufficient to render labour performable, by a less exercise of motor force than usual. The existence of dilatation is at once an indication of the presence, and a measure of the extent, of the failure of the spinal motor powers in parturition.

Observation 24. It has been seen experimentally, that the peristaltic action of the uterus continues vigorous after the total annihilation of the direct and reflex action of the spinal system. It is this power which effects delivery, in some cases, in the human subject in paraplegia, and even after death, when at least the actions of respiration, and of the heart, have entirely ceased. In cases of labour proceeding under the influence of Chloroform, when the anæsthetic is pushed to the extent of enfeebling the spinal function, it is, as I believe, the peristaltic action of the uterus which, in the great majority of cases, is then the principal agent in delivery.

Observation 25. Thus, in anæsthetic midwifery, the natural arrangement of the motor powers appear to be modified in the following manner. The reflex function, as concerned in parturition, is affected in two modes. 1. The reflex actions are weakened, so that the impression of the advancing fœtus upon the excitator nerves of the parturient canal does not excite their wonted reflex actions to the same extent as usual. The spinal *contractions* are below the average. 2. On the other hand, all the parts concerned in the dilatation of the parturient passage and outlet, are in a state of relaxation, depending upon the condition of the spinal function. The spinal *dilatations* are increased by the influence of Chloroform. While this state of things obtains, the peristaltic action depending upon the ganglionic system remains in the ordinary state of activity, and in the relaxed state of the passages, is sufficient in many cases to effect the delivery of the fœtus, without any unusual delay. This dilatation of the parturient canal also exists in certain cases of paraplegia, and even after death; so that the occurrence of delivery during profound anæsthesia—the occurrence of delivery, in some cases, in experiments in which the lower portion of the spinal marrow is injured or destroyed—the occurrence of delivery in the dead subject,—are no proofs whatever that the spinal marrow is not actively concerned in natural parturition, or that peristaltic action is the sole power by which the fœtus is expelled in natural labour in the human female, where the dilatation of the parturient canal is commonly a painful process. Delivery under Chloroform, appears, then, to be a pathological condition, in which, as regards the motor actions, a state of artificial dilatation compensates, in some degree, for the deficiency of expulsive power. From these data, as it appears to me, we must survey the motor derangements which are declared sometimes to occur in the practice of Chloroform.

Observation 26. The facts relating to the destruction of the spinal functions, including the respiratory function, tend to show, that in the treatment of persons dangerously affected by Chloroform, the attempt to excite artificial respiration alone is not likely to be attended with success; but that the peristaltic action of the heart should be kept up

by galvanism, and other direct stimuli, until, if possible, the poison shall have been eliminated from the spinal system. It appears evident, that after the reflex movements of respiration have ceased, the heart may still act; and to this organ attention should, I think, be chiefly directed in the attempts at resuscitation in accidental poisoning by Chloroform.

Observation 27. The experiments detailed, furnish some important obstetric hints of a practical nature. It has been recommended by the advocates of anæsthesia, that the ergot of rye should be given with Chloroform, in certain cases, in order to strengthen the actions of the uterus in tardy labours. A little consideration will show, however, that the ergot is not likely to be serviceable in uterine inertia, complicated with Chloroformization. There can be no doubt, that the ergot itself acts as a spinal excitant, sometimes even producing general tetanic movements in the limbs of the parturient woman. It is little better than a contradiction, to give a spinal *stimulant* and a spinal *sedative*, at one and the same time. The true indication in this form of inertia would be, to stimulate the peristaltic action of the uterus by galvanism, or by direct mechanical irritation. The appeal should be made, not to the spinal function—at least not while the influence of the anæsthetic continues—but to the peristaltic action of the uterus, which remains unimpaired by the Chloroform, after the spinal function has been enfeebled.

Observation 28. In uterine hæmorrhage after delivery, occurring under the use of Chloroform, the knowledge of the peculiar way in which this agent affects the nervous system, supplies important indications of treatment. Reliance should not be placed upon the administration of ergot, or the excitation of uterine reflex action by cold and other means; but on mechanical pressure, the galvanic stimulus applied to the uterus, and direct mechanical stimulation of the organ. In the arrest of uterine hæmorrhage under Chloroform, there can be little doubt, that we have the serious disadvantage of the loss of one of the most efficient motor powers for its arrest. Similar considerations also apply to those labours and obstetric operations, in which a diminution of the motor force of the uterus is of temporary advantage, and in which a safe and manageable sedative of the utero-spinal action would be an invaluable boon; but to enter fully upon these matters on the present occasion, would occupy more space and time than I can either claim or afford.

Observation 29. The principal conclusion deducible from the experiments detailed in the present paper, and warranted by the experience of Chloroform in midwifery, upon which I would insist, is this,—that Chloroform enfeebles the action of the spinal marrow and the reflex function, *pro tanto*, according to the profoundness of the anæsthesia induced, but that it leaves the peristaltic actions little, if at all, affected, so long as life continues. Besides the influence of volition and emotion, the action of the uterus in PARTURITION is both REFLEX and PERISTALTIC; the peristaltic action being, as I have insisted, in my work on *Parturition and Obstetrics*, “the basis of the other uterine actions”,—“the basis or radical element upon which the other causes of motor action operate”; and I quote these words to show that I have not underrated the importance of uterine peristaltic action. Setting aside natural labour, abnormal parturition may be divided into two great classes,—

1. Parturition with diminished motor action. 2. Parturition with excessive motor action. The study of the relation of Chloroform to these two classes of difficult labour, and to the different forms of motor power concerned in natural and difficult parturition, is scarcely less important than its study as an anæsthetic. I have attempted to supply the indications according to which this study should be pursued.

NOTE. In the discussion on the employment of Chloroform in midwifery, in the course of which the experiments detailed in the preceding paper were briefly alluded to,¹ Mr. Barlow is reported to have observed as follows: "He had heard it said that the production of anæsthesia in cases of midwifery, had shown that the uterus was independent of the spinal cord. They showed nothing of the kind. How could it be said that the influence of the cord was withdrawn from the uterus, as long as breathing continued? Was it possible to destroy the function of the cord below, and yet leave the medulla active, there being only one circulation for the whole cord?" Fully coinciding with the first remark of Mr. Barlow, I would refer to Observation 5, as giving the answer to his two most important interrogations. The function of the medulla oblongata *does* appear to continue, after the rest of the spinal marrow has died. I find that a confirmation of this observation may be drawn from a detail² of some excellent experiments performed by Dr. Sibson, but without any reference to uterine action. Dr. Sibson says, "As the action of Chloroform increases, costal respiration ceases, and the diaphragm only acts; the functions of the spinal marrow being gradually destroyed *from below upwards*." This is in strict accordance with what I have observed, and the mode of dying peculiar to the spinal marrow, is very important with reference to the influence of Chloroform upon the functions of this organ, and especially with reference to the uterine reflex actions.

Bolton Street, Piccadilly, November 1849.

¹ Vide Report of Proceedings of Westminster Medical Society, Oct. 27, and Nov. 3.

² Medical Gazette, February 18th, 1848.

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32. THE CHOLERA CONSIDERED PSYCHOLOGICALLY. By FORBES WINSLOW, M.D. pp. 15. London: 1849.

In concluding a hasty notice of various works on CHOLERA in our April number, p. 346, we promised to review the other memoirs which were then threatening to issue so abundantly from the press; and we now proceed, in some measure, to redeem that pledge. The more important detached memoirs and observations, which have appeared in our own or in foreign journals, have already been either transferred to, or noticed at sufficient length in, the LONDON JOURNAL OF MEDICINE, and we have therefore only transcribed above the titles of such essays as have reached us in an independent form. After giving a brief description of the different works before us, we will endeavour to embody, in a few simple propositions, the sum of our knowledge of Cholera. The principal deficiencies, and the most likely means of supplying them, will then suggest themselves to our readers.

DR. ALCOCK opens with a long address to the public, for whose enlightenment his book has been chiefly composed. There is also an address to the Dublin Board of Health. The author is a non-contagionist. "I have acquired", says he, "a thorough disbelief in the contagiousness of Cholera; for

I am certain, that when Cholera reaches any locality, it is equally diffused through that locality ; but, like the hovering eagle, makes its stoop upon its easiest and surest prey : hence its fatality amongst the poor and profligate. Our worst cases came from Leeson Lane,—yet I never heard of any case occurring in Leeson Street”, p. 54. The choleroid diarrhœa, Dr. Alcock treats with pills of acetate of lead and opium : this we regard as sound in principle, but the same medicines (in our experience), administered in small enemata, are more rapid and certain in their effects. Minutes are often precious. A little more serous hæmorrhage from the bowels may make all the difference possible, in the issue of the case. Blood-letting the author appreciates at its proper value,—that of a mechanical remedy which may relieve congestion, but is otherwise fraught with danger. The pathology and treatment of Dr. Alcock, we may say (in a general way), are sound. His anti-contagion arguments have not convinced us.

The work is hardly entitled to be called a Treatise on Cholera, as it is rather a series of fragmentary essays, commencing with “an address to the public”, and ending with a history of “the Tooting massacre, or murder of the innocents !”

DR. JAMES BIRD’s pamphlet is one of the best of the publications called forth by the recent epidemic ; and the only error of the author, of which we have to complain, is his brevity. We should like to see a complete history of Cholera by Dr. James Bird. Such a work is required, and there is no one better able to execute the task. As almost the whole of this pamphlet appeared in our pages, we pass on to the next on our list.

DR. G. G. BIRD’s brochure being intentionally adapted to the general reader, is hardly within the sphere of our criticism. So many very questionable medical works are concocted for the unprofessional part of the community, that we think it right to say, that Dr. G. G. Bird has not transgressed the bounds of propriety, but has really produced a pamphlet likely to be useful to those who are unable to master more learned treatises.

The author, we observe, complacently adopts as “historical illustrations” matters of controversy. He speaks, for example, of the “Irish typhus” as “a new epidemic which appeared first in 1846”: and he states that small-pox and measles first showed themselves in the sixth century, and scarlet fever early in the seventeenth century ; that syphilis became first known about the year 1494 ; and that the present malignant Cholera first appeared in 1817”. (p. 17). We beg leave to say, that these assertions are not in accordance with the researches of many learned men ; and that, in place of being historical illustrations, none of them are more than hypotheses.

DR. WILLIAM BUDD issued his little work in hot haste, almost simultaneously with the famous announcements of Drs. Swayne and Brittan, which we have already very fully laid before our readers. His speculations are entirely based upon the unconfirmed discovery of special Cholera bodies by these gentlemen, with whom Dr. Budd was associated in the investigation, instituted by the Bristol Medico-Chirurgical Society, into the pathology of Cholera.

We find it stated (p. 9), that, “in the rapidly fatal cases in which no fluids are ejected from the body, there is always a large *outpouring* of fluid into the intestine, although this fluid may not be voided. Death, here, in fact, is the exact counterpart of death by internal hæmorrhage”. If this phenomenon be “always” present in the rapidly fatal cases, it is not noticed by those authors who have most minutely described the pathological anatomy of Cholera. From *à priori* reasoning, we think it very probable that Dr. Budd may be correct in his statement ; but, in the mean time, it stands in need of confirmation by the detail of many dissections.

MR. JOHN GROVE relies greatly on SULPHUR in the treatment of Cholera : in fact, he almost announces it as *the specific*. “Should it be proved”, says he, “as I anticipate, that sulphur is an antidote to the Cholera poison, it is

not unlikely that the same remedy may be available for other epidemic diseases" (p. 25). We have no doubt whatever, from the facts furnished in Mr. Grove's pamphlet, and from others which have come to our knowledge from entirely different, and most impartial, sources, that, in the earlier stages of Cholera, this medicine may be productive of great benefit; but we are far from being convinced that it is more than one of several therapeutic agents, of which the same may be said. Mr. Grove gives, in a tabular form, 101 cases of "Cholera and Choleraic Diarrhœa" treated by Sulphur; and the deaths were three in number. It is stated, that "none of the cases recorded in these tables were of a simple nature; in each there was some peculiarity which gave rise to alarm and anxiety". We have no doubt that benefit arose from Mr. Grove's treatment; but nothing more than a general impression of its success is conveyed to our minds by his statistics, as we have found, by careful enquiry among our friends, that the ratio of recoveries, in any given number of cases, depends more on *the diagnosis*, that is to say, upon the principle in which certain cases are embraced in, or excluded from the lists, than upon any other cause. Every method of treatment has been statistically proved (?) by somebody or another, to be the best; and not a few methods have, in the same way, been made out as hardly ever known to fail. It is, therefore, not from the *numerical argument* of Mr. Grove, that we think well of Sulphur in Cholera, so much as from the *general impression* which the pamphlet conveys, that Mr. Grove is a sound and competent observer of disease, and not likely to be greatly misled in a matter of this kind.

Collateral circumstances strengthen us in our opinion. We have now before us a letter, dated 22nd September, 1849, about business matters, written by a gentleman in Edinburgh to a physician in London, to which the following postscript is appended:—"I saw a man to-day from the south muirs,¹ who told me, that his wife and he, as well as many of the people of his remote landward parish, had been very unwell; and, from the symptoms which he mentioned, it was clear that they had suffered from Cholera, or had been saved from it. They had no medical man near them, and got no medical attendance, yet there were no deaths. As soon as they found cramps come over them, they took a teaspoonful of powdered brimstone, or, sometimes, the flowers of sulphur, mixed with a little whiskey, to which was added water, if the sick could not otherwise swallow the dose. The man described the cure as certain, and very rapid. Try this on the London folks; it may serve the afflicted, and do you much good". This accidental non-professional testimony seems worthy of notice.

Mitchell says that, "the sulphureous localities of the sickly island of St. Lucia are its only salubrious places. Cities, too, which abound in sulphur products . . . enjoy an immunity from ague, for which they are everywhere noted."² Immediately around the sulphur works, and factories for making gunpowder and sulphuric acid, the vegetation and the ague disappear together". To this we may add, that, this year, during the bombardment of Rome, that city suffered much less than usual from malarious fevers,—a fact which induced Mr. Walker to recommend cannonading, and the discharge of fire-works, as measures likely to ward off, or prevent the spread of, Cholera. In our number for April (p. 345), in reviewing Mr. Blacklock's pamphlet (Madras 1848), we did not entertain with favour his theory of the disease, and his sulphur treatment; but the perusal of Mr. Grove's pamphlet, and the facts at which we have glanced, lead us to believe, that though Mr. Blacklock's theory be untenable, there may be some value in his therapeutical and hygienical dogmata. The following are his ideas:—"I hope yet to see the day when sulphur, in small quantities, will be regularly issued to every soldier in the field, in India, say forty grains per day, while actually marching, and twenty grains per day, during halts, as a sure way of warding off

¹ Sheep pasturage in the south of Scotland, very thinly peopled, and ill drained, we believe.

² And what is Cholera but a pernicious ague?

this terrible disease ; and I have a firm belief that sulphur, so employed, will be as effectual in banishing Cholera from our armies, as lime-juice has been in eradicating scurvy from our fleets”.

Mr. Grove quotes the following curious passage from the *London Practice of Physic*—a work published in 1692 :—“ In the year 1670, about the autumnal equinox, a world of people here were seized with a most dangerous flux (though without blood), and joined with a cruel vomiting, which presently caused great faintings and a total decay of strength. For the cure of this disease, no evacuation did good ; nay, bleeding, vomiting, and purging, did hurt, only cordials, and those of the hottest nature, to wit, such as abounded with *spirit and sulphur*” (p. 22). From this, it would appear that the whiskey and sulphur treatment of the south muirs of Scotland is an old tradition.

Manec administered sulphur in all possible forms, in his treatment of Cholera in the Salpêtrière, as we stated at page 673.

Sulphur is a stimulant of the capillary circulation, and so is camphor—one of our best remedies against collapse. Perhaps the combination of the sulphur with hydrogen is the cause of the augmented heat in the surface of those who take the former in repeated doses. The characteristically offensive smell of sulphuretted hydrogen gas is proof sufficient that this chemical action does take place.

DR. KNOX has digested and arranged a great mass of information, and has thus produced a most interesting and instructive volume. As it appeared in January last, the great and important additions which have, since that period, been made to the literature of Cholera, are not embodied in the work ; but almost everything of value which appeared before that date, has been noticed. The first chapter is a history of Cholera, from which it appears that it is *not a new disease*. The second chapter contains an account of the morbid anatomy, which is a fair summary ; but no summary of observations made by different individuals, in different circumstances, can convey very precise ideas on this subject. Chapter third gives the pathology, with reference to the proximate cause of the disease. The author thus enumerates his conclusions :—

“ 1. By the operation of various causes, either singly or in combination, a morbid impression is primarily made on the mucous membrane of the stomach and intestines.

“ 2. The first effects of this impression are manifest in a general feeling of indisposition, change in the expression of the features, languor, præcordial uneasiness, nausea, diarrhoea, and other marks of derangement of the digestive apparatus, generally described as preliminary signs of Cholera.

“ 3. The symptoms there described may be, however, more correctly classified together as the first stage of the disease, tending, if unchecked, to be succeeded either simultaneously, or in succession, by biliary, and then by sero-albuminous discharges from the alimentary canal.

“ 4. As effects of the evacuations in question, conjoined, possibly, with a direct morbid impression made on the nervous, and perhaps the circulating, system, may be enumerated, alteration in the quantity, consistence, chemical composition, and colour, of the blood ; arrest of the secretions ; spasms, varying in kind, situation, and intensity ; diminution of the animal heat ; lividity of the surface ; internal congestion of blood ; failure of the various functions ; collapse ; and, where the disease continues unabated, death.

“ 5. Not as a direct result of, nor essentially forming a stage in the disease, but depending on the weakened condition of the system, and the general derangement of its various functions, re-action, in different forms and degrees of intensity, may take place, assuming variously the type of bilious or typhus fever, or passing into a congestion and inflammation of the lungs, brain, and digestive apparatus, either separately or conjointly.

“ 6. Cholera does not, like typhus or eruptive fevers, run through a deter-

minate course, but may be arrested, at any stage of its progress, by the efforts of nature, or by the judicious application of remedies.

"7. It may occur either sporadically, or amongst large numbers at once, the frequency of its occurrence, and its intensity, being determined by the conjoined effects of some epidemic influence not yet understood, and of varying degrees of individual susceptibility." (pp. 53-54.)

Our limited space forbids criticism, otherwise we would pause to examine the above conclusions.

The *Symptoms of Cholera* are described in detail in the next chapter. The following passage regarding the influence of the disease upon *Puerperal Women and the Fœtus in Utero* is interesting; though we rather think some of the statements are not applicable to all localities. "In the Cholera Hospital at York, a great number of females, affected with the disease, continued to give suck; and in a patient at the St. Pancras Hospital, the secretion of milk did not cease, although the urine was suppressed, and miscarriage took place. The disease usually, if not always, proved fatal to the fœtus, although Kohl alludes to more than one infant carried to the full time. In several children, delivered by him immediately after the death of the mothers from Cholera, there was not the slightest evidence of life; and he always found the body, as well as the placenta and umbilical cord, to be exceedingly pale and bloodless, although the temperature of the interior of the vagina and uterus was in every case extremely high. He also observed, that the placental murmur became more feeble, and usually less and less obvious as the pulse sank; but it was rendered at once more powerful and distinct by blood-letting. The fœtal circulation generally became more rapid and feeble with the progress of the disease." (pp. 59-60.) It is a pity that we are not informed of the number of cases, and of the names of the observers upon whose authority these generalities are given. Here, as in much that has been lately written about Cholera, we have to complain of a disregard of precision, where absolute statistical accuracy should have been attempted.

Relapses are thus spoken of:—"From the united testimony of Brown, Gendrin, and many others, it appears that relapses were much more frequent in Europe than in India, or at Orenburgh. Indeed, in Bengal, as we are told by Jamieson, a second attack was a rare occurrence, whilst, in the practice of Mackintosh, they were very frequent; and the later testimony of Thom, from Scinde, is to the same effect: some of the men having had three attacks before leaving the hospital, of which the last was the severest."

This question, as to the circumstances in which relapses occur and do not occur, requires elucidation. We are not aware of any author who has attempted an analysis of the facts. So far as the imperfect data which we possess enable us to come to any opinion, we are inclined to think that the same treatment, hygiene, and change of air, which prevent the repetition of a paroxysm of intermittent fever, will, in essence and principle, be found to be those which avert, or soften the recurrence of the Choleraic paroxysm,—which, in cases sufficiently moderate to admit of adequate observation, presents the same stages as the former, though generally in a more pernicious form. We think, however, that an examination of the subject of "*relapses*" is much wanted; as, besides its independent interest, it would tend to clear up and establish some points in the etiology of the disease.

The remarks on *Diagnosis* are brief, and most unsatisfactory. In fact, nothing of value can be said on the subject, till an enlarged view be taken of all cases of diseases prevailing during an epidemic, and their various relationships be carefully studied. An immense mass of valuable information has been lost by individuals setting up a picture of Cholera, and excluding from the facts on which their conclusions are founded, every case which did not correspond with their fancy formula of symptoms. They have in this way virtually expressed an opinion, and then selected facts wherewith to support it! It is within our own knowledge, that the Cholera statistics

edited by the Registrar-General, are to a great extent deteriorated by this absurd system; and the observations which practitioners of repute and good judgment retail in conversation, in societies, and in print, show that very few indeed have had their eyes open to the importance of studying the natural history of the milder Choleroïd affections (with which almost the whole population of the southern bank of the Thames were affected, who escaped the graver form of the epidemic). The symptoms of complex cases of disease are best studied and analysed, first in mild cases, and then in groups, varying in the arrangement and severity of their phenomena. The vomiting, the purging, the cramps, the suppression of urine, and the pulmonary oppression, might all, two months ago, have been examined in a scale of cases ascending from the most trifling to the most awfully malignant and uncontrollable; but the fashion was to say,—this is not Cholera,—that is not Cholera,—this is not “Asiatic” Cholera, and so on. In this way, the symptoms have been little analysed; and the attention of the profession has been chiefly directed to that form of the disease in which they are too severe and too complicated to admit of individual scrutiny.

The *Prognosis* is unfavourable, we are told (chapter sixth), in epidemic Cholera, “Dr. Wright having calculated, that of one hundred million of cases occurring since 1817, one half have perished”. It need hardly be observed, that the comparative mortality from Cholera varies with the fancy of each reporter; for what one man calls Cholera, another maintains to be something else. In this, as in most subjects of medical inquiry, the actually available statistics are miserably scanty. Those now before the profession with reference to Cholera, would lead to various and contradictory conclusions, most of them being very far indeed from the truth. In fact, the results would be of this kind:—few or no deaths if Dr. Ayre’s treatment be adopted and properly carried out: every case fatal in which Dr. Ayre’s system was tried: mortality very trifling in cases treated by Morrison’s pills, hydropathy, or homœopathy. Notwithstanding what we have said, the elaborate tables given by Dr. Knox of “proportional attacks and mortality in various places” are possessed of much interest.

The *Etiology* of Cholera is treated of, in the seventh chapter, at considerable length. Natural susceptibility, predisposing and exciting causes, atmospheric influence, mineral effluvia, vegetable sporules, electricity, planetary influence, meteorological phenomena, malaria, and the contagion-question, are severally discussed. Through all the facts and reasoning on them, it is impossible to follow the author; but we may, nevertheless, say that the chapter is carefully compiled, though (from the date of publication) deficient in recent facts. We can agree with the author, in saying that the diffusion of the disease depends on unknown causes; but we cannot admit that all the causes of diffusion are unknown; nor can we, in the face of the history of the last epidemic, and with the facts detailed in the Indian reports, adopt Dr. Knox’s conclusion, that Cholera “has not been proved to be contagious.” We adopt the opinion, which is every day gaining strength, that its spread depends upon several influences—one of which is communication with the sick.

Prophylaxis, in chapter eighth, is discussed under the heads of temperance, food, cleanliness, abundance of light and air, chemical agents, quarantine, and removal from an infected district. Removal to a healthy district,—especially to a dry and airy situation,—is truly regarded as the most effectual means of prophylaxis. “Many examples”, says the author, “may be found scattered through the history of the epidemic; but a few instances will suffice to illustrate the point. The first to which we shall refer occurred in the army of the Marquis of Hastings, in India. When nearly ten thousand men had perished within a very brief period, directions were issued for a change of quarters; and the troops having moved forwards a few days’ march, broke fresh ground on the banks of a healthy stream, when the disease abated,—and that suddenly, in the most remarkable degree. Kennedy, however, in

adverting to the circumstance, alleges that the epidemic was subsiding before the army moved from their first encampment, but rather inconsistently admits, that, in the line of march to their new position, the road was covered with the dying and the dead. Again, when the epidemic was raging at Gibraltar, a regiment which marched out to cantonments on the Spanish lines, escaped; and, during the virulence of the pestilence at Aleppo, Lesseps (the French consul there), in company with two hundred persons, retired to a garden situated at a short distance from the city, closed within walls, and surrounded by a large fosse; and, as long as the malady lasted, nothing was admitted into the place, without all the precautions observed in lazarettos. In this case, not one of his followers was attacked by the disease, which, meanwhile, destroyed in the city upwards of four hundred souls. Similar measures were followed by like results in Persia, as detailed in a letter from M. Gamba to Baron Larrey. It is true that the immunity alluded to has been attributed to the simultaneous adoption of complete restriction of intercourse with the infected; but the same results were apparent where no precautions whatever were taken, beyond change of residence; and that this was the true cause of safety, may be inferred from the fact, that, throughout the progress of the epidemic, its progress has been principally confined to particular localities, beyond the limits of which the inhabitants suffered little or nothing from its ravages. This peculiar feature in the disease was well exemplified in the escape of one side of a street, one division of a barrack-square, of particular regiments in the field, or even in different lines of the same regiment in India; and many similar instances occurred in Europe." (pp. 179-80.)

"As the only rational prophylactic measures, these may be briefly recapitulated,—strict temperance in all things, personal and domestic cleanliness, early attention to the first symptoms of indisposition, during the prevalence of the epidemic, removal, where duty will admit of it, from an infected to a healthy district, and above all, a cheerful and patient resignation to the will of that Divine Being, who doeth all things well, and will not unnecessarily afflict His erring creatures." (p. 183.) In addition, we would suggest that all should be kept in the most vigorous condition possible, by the regulation of diet, exercise, and sleep, assisted in a large proportion of the population by such special medicines as iron and quinine, separately, or in combination.

The various remedies and classes of remedies which have been recommended or tried, (embracing the recorded practice of some hundred practitioners,) are described and compared in the ninth chapter. This is a very useful and interesting part of the volume; and we regret that we cannot afford space for more than one short extract.

"On the whole, we question whether the recorded *Results of the Exhibition of Mercury* will enable it to retain its position in the opinion of the profession generally, as being superior to all other remedies, and fully concur in the view expressed by Dr. Sharkey, that it had no specific action whatever in rallying a patient from collapse. The horrible ptyalism sometimes produced by it has been also urged as an argument against its employment; but if this drug were really possessed of the curative virtues attributed to it by Dr. Ayre and others, this objection should have little weight; when reference is had to the very deadly nature of the disease to be combatted. It is, however, difficult to determine precisely, on what the alleged power of calomel over the malady depended; for it was administered in quantities quite too large, and too frequently repeated, if employed merely as a purgative; and ptyalism is conceded not to have been necessary to a cure, the recoveries being as frequent without as with this specific effect. Indeed, Dr. Ayre, one of its staunchest advocates, admits that, during collapse, no absorption of the drug takes place. And this is evident from the cases on record, masses of calomel being found lying unchanged in the alimentary canal after death; and when we read of its being administered in large

quantities varying from five to fourteen hundred grains in the course of a few days, without inconvenience, we are naturally led to conclude, that, fortunately for the patient, it has been almost if not entirely, inert. *What result might be anticipated from the absorption of a quarter of a pound of calomel, administered within a few days to a single patient?* With reference to the opinion here expressed, we may state that Cumming, when in India, was frequently able to collect this drug from the stomach after death, precisely in the state in which it had been swallowed, not having even come in contact with the inner coat of the stomach, from which it was intercepted by a viscid secretion which lined the mucous membrane. In dismissing the subject, it may be stated, that to sanction the treatment of Cholera, by the administration of enormous quantities of a drug so powerful as calomel, more will be required than general assertions of its utility, however strong; nor should it be adopted, except on the most satisfactory statistical demonstration of its superiority over safer and more rational methods of cure. I would not, however, be understood to proscribe the legitimate use of mercury in this disease, but merely to protest, as strongly as the importance of the subject demands, against what seems to me to be a dangerous abuse of a very valuable medicine." (pp. 205-6.) The fact for which Dr. W. F. Cumming is cited as the authority, is detailed in an interesting, racy, and most original book of travels published in 1839, by that gentleman, under the title of "*Notes of a Wanderer in Search of Health.*"

The leading objects of treatment are thus summed up by Dr. Knox.

- "1. To tranquillize the irritation of the stomach and intestines, and appease the distressing thirst.
2. To arrest the discharges.
3. To allay spasm, and tranquillize pain, and other nervous symptoms.
4. To restore the secretions of the liver and kidneys.
5. To keep up, or recruit the circulation and natural heat of the system, and support the strength.
6. To moderate the fever, and counteract the tendency to visceral inflammation in the re-active stage.
7. To adopt such measures as may hasten convalescence, and prevent the danger of collapse.

These indications, it will be seen, simply resolve themselves into the adoption of measures for the prevention or counteraction of collapse, and the management of fever, when that may happen to supervene." (p. 232.)

A Chronological Bibliography of Cholera occupies the twelfth and last chapter. This, with the notes appended to each chapter, the author correctly believes to be "the most complete bibliography of Cholera in the English language." By adding to it, references to the books and papers which have appeared since his book was published, nearly a year ago, Dr. Knox would make this important department quite perfect.

The Appendix contains formulæ of astringent, sedative, antispasmodic, stimulant, aperient, diaphoretic, tonic, and diuretic medicines; and also a short account of the Cholera in 1848.

The work of Dr. Knox we leave with reluctance. From the clear and sound judgment which it displays, and the extraordinary amount of well-digested information which it contains, it must take a permanent place among the classics of our medical literature.

SIR JAMES MURRAY'S publication is wildly hypothetical, and sadly destitute of applicable facts. The following sentence, however, does contain a seasonable truth. "The pathological consequences of electrical alterations are not yet sufficiently ascertained to admit of mathematical demonstration." (p. 50.) The following is the expression of a hope: "I have much reason to believe, although not yet able to prove the fact, that positive electricity bears a relation to *active ailments*, and negative electricity to *passive complaints*." (p. 61.)

DR. PARKES' pamphlet is a very valuable contribution to the facts by which the question of contagion has to be tried. The reasoning is excellent. "As far as we can at present judge, from what is known regarding the spread of Cholera in Great Britain, there appears to be much evidence in favour of the position, that this extension has occurred in two ways, most commonly by independent manifestation in particular localities, but sometimes by transmission through the medium of diseased persons." (p. 26.)

DR. SHAPTER has made a splendid contribution to the history of Cholera, and to the medical topography of England.

MR. HENRY STEPHENS trusts to Calomel: but why, we cannot discover. The following passage refers to some medicines which are, in certain circumstances, useful. "To those medical men who have not the same faith and confidence in the power of calomel as I have, I would suggest a trial of the following agents, each of which, by its effect of coagulating albumen, I should expect to be beneficial in the early but threatening stage, when there are 'rice-water' evacuations: alum dissolved in water; creasote in pyroligneous acid; nitrate of silver, in the proportion of one grain to the ounce; perchloride of mercury, in doses of half a grain, dissolved in four to six ounces of water. These may be combined with laudanum, in 20 to 40, or 60 drops for a dose. The effect of the nitrate of silver in coagulating albumen, and changing the appearances of diseased surfaces in external sores to a healthy appearance, would lead me to expect much from it. But it must be borne in mind, that in simple diarrhoea, opiates, with essential oils and creasote, will cure the majority of cases; but when the diarrhoea resists these means, I would try either the perchloride of mercury, or nitrate of silver; and should the symptoms still continue and cause alarm, I should affect the mouth as speedily as possible with mercury, and probably this could be done more readily with the corrosive sublimate than with calomel." (p. 45.) We quote the concluding paragraph: "I am impressed strongly with the value of the principle I have suggested, that to antiseptics we must look for the curative means of Cholera, and not only of Cholera, *but of all malignant fevers*; but I have still this conviction, that if we find a remedy which will arrest Cholera with certainty, it must be before collapse is complete. When that stage is fully established, the Disease will be found to have triumphed, and Art must resign the contest." (p. 47.)

DR. SNOW'S interesting views have been already fully brought before our readers in the reports of the Westminster Society, pp. 1077, *et seq.* They are true to some extent, and have given a useful bias to one part of the inquiry into the mode in which the disease is communicated.

DR. WINSLOW put forth his brochure very seasonably, in the midst of the panic. It originally appeared as a newspaper letter. The author says—"a humble reliance on the will of God, a well-sustained piety and cheerfulness of mind, combined with a careful avoidance of those physical agents known to have a deleterious influence on the health, are the safest and most legitimate means of protecting us from this terrible malady. It is our duty, individually as well as nationally, to fortify and strengthen the system by resolutely determining not to yield to useless fears, and childish apprehensions; and as far as it is in our power, to inspire ourselves and neighbours with energy and courage: and as a powerful prophylactic agent, to cultivate

'Sweet unanxious quiet for the mind!'

We have now given an account of thirty-two separate publications on Cholera, in addition to the numerous articles which we have given entire or abridged in our Digest of the Journals and Reports of Societies. To some readers, we fear, the devotion of so large a proportion of our space to one

subject may have been unacceptable; but as it is at all times our object not merely to give variety to our pages, but also to make each volume of the LONDON JOURNAL OF MEDICINE a library book of permanent value, we have judged it necessary to surrender as much space to the subject of Cholera, as would fairly allow the whole of our actual knowledge of the disease to be laid before our readers, or the sources where it is to be found accurately pointed out. We have omitted notices of most of the alleged specifics which have been so senselessly and so clamorously obtruded upon the public, and the profession, by some of our brethren, in the weekly medical journals, and even, we regret to say, in the daily newspapers.

We have preferred giving in detail, clinical facts, and the opinions of competent observers—even at the risk of wearying by sameness and repetition—rather than offering our own views in the form of commentary or criticism. It seems proper, however, to state the CONCLUSIONS at which we have arrived, from the careful study of all that we have seen and read, not with the pretence that they are unimpeachable dogmata, but simply as a series of truth-like propositions, whereby each student may be assisted in examining the data for himself.

1. That Cholera is a fever, intimately related to those fevers which depend on malaria.
2. That the intermittent or remittent type can be generally recognized in the milder, and also not unfrequently, (though less distinctly,) in the severer cases.
3. That the stage of collapse ought to be considered as an aggravated cold stage of the paroxysm of a pernicious fever, which may spontaneously terminate in death or reaction.
4. That the least dense portion of the blood has an excessive tendency to exude through the capillaries of the stomach and bowels, and pass from the body by vomit and stool.
5. That the inspissated residual blood being unable to pass through the small pulmonary vessels causes congestion of the lungs; and as speedy consequences of this condition, paralysis of the right side of the heart from over-distension, asphyxia, and other subordinate derangements of the vital actions.
6. That death may take place from
 - a. Asphyxia.
 - b. *Necræmia with* loss of the least dense portion of the blood by stool and vomit.
 - c. *Necræmia without* such loss of the least dense portion of blood as can be discovered during life—the exudation remaining within the stomach and intestines.
 - d. Toxæmia from absence or deficiency of sanguineous depuration.
 - e. Inflammation of lungs or other organs supervening in convalescence.
 - f. Debility.
 - g. Gastro-enteritis.
 - h. Two or more of the above causes combined.
7. That the anatomical lesions found on dissection vary with the causes of, and circumstances attending, death.
8. That the *treatment* cannot be reduced to any routine formulary, but ought to be adapted to the particular condition of each patient, and to the stage of the disease: that the “rice-water” vomit and purging require to be energetically subdued by acetate of lead, creasote, nitrate of silver, and such like suitable remedies: that, in threatened collapse, the hot wet blanket, stimulant embrocations, stimulants which act on the capillaries (camphor and sulphur) are of signal benefit: that in reaction, and during convalescence, local inflammations, and congestions, require to be guarded against or subdued, and rational means adopted to restore the secretions of the liver, kidney, and skin, but

particularly of the two former; and, lastly, though not of less importance, that the character of the fever be modified, and a repetition of the paroxysm be guarded against, by change of air, or by the administration of quinine, which, in the majority of cases, from the existence of anæmia, ought to be conjoined with iron.

9. That the *prophylaxis* of Cholera consists in removal to a better air, improving the sanitary condition of localities and individuals, and in the exhibition of the disulphate of quinine twice or thrice daily.

LIFE AND WRITINGS OF THE LATE JOSEPH CLARKE, M.D., formerly Master of the Dublin Lying-in Hospital, etc. etc. : Containing minute results of his Private Practice, extending over a series of forty-four years, including 3,878 births. By ROBERT COLLINS, M.D., President of the King and Queen's College of Physicians in Ireland, formerly Master of the Dublin Lying-in Hospital. pp. 88. London: 1849.

This tiny volume is a most valuable addition to obstetric literature and medical biography. We regret, however, that Dr. COLLINS has given us so slight an outline of the private life of his illustrious father-in-law, and so few specimens of his correspondence. Doubtless, the life of a busy and successful Dublin physician could furnish few passages of vicissitude or stirring incident; yet from the information contained in the volume before us, we cannot help thinking that Dr. Collins has erred, in withholding much which would have been both interesting and instructive. We trust this small volume is but the foretaste of a complete memoir.

The sketch commences with the following AUTOBIOGRAPHICAL FRAGMENT, found attached to the last page of his first fee book: "I am told that I was born on the eighth of April, 1758, in the parish of Desertlin, townland of Tamnadoey, County of Londonderry. My father, James Clarke, had realized some property, chiefly by agricultural pursuits. His character was such as I shall be ever anxious to emulate: his reading was not extensive, being principally confined to the Bible. I was a second son, and very early in life showed a great disposition towards school learning. At the age of twelve, I was put to a Latin grammar school, being then a good English scholar, clerk, and arithmetician. I remained nearly five years with a curate (Kearns), who had been educated at Trinity College, Dublin; and was always reckoned one of the best scholars of my standing. After reading Euclid's Elements, my grand-uncle, by the mother's side, Dr. Maconchy, of Dublin, recommended Glasgow College to me for the study of logic, mathematics, Latin and French, each of which I prosecuted with industry and sensible improvement, in the winter of 1775 and '76. Thus prepared, I went to Edinburgh for the three ensuing years, where I studied medicine with a regularity and zeal exceeded by none. In September, 1779, I graduated with great ease to myself, and some reputation. Soon after my return to Ireland, I visited my friends in Dublin, where I received a pressing invitation to remain some time in Dr. Maconchy's house. Having spent about three months in this city, much to my satisfaction and advantage, I determined to leave it, with the intention of settling as a physician in Derry. A friend being in pursuit of commendatory letters to forward me in this object, heard of a companion being wanted to accompany Mr. N. L. Rowley's second son to the Continent, whither he was going for the improvement of his health. Having been fortunate enough to obtain this appointment, I set out on the 3rd of March, 1780, for Lausanne, in Switzerland, where we spent the summer months very pleasantly, while Mr. R.'s health was tolerably good. In October, however, such a change had taken place, as made it necessary for us to return to London, where I availed myself of the opportunity thus afforded me of attending Dr. William Hunter's spring course of lectures, which included those on the gravid uterus, and from which I derived several useful hints and practical cautions. In May, 1781, Mr. Rowley died, upon which I returned to Dublin. During my ab-

sence, my respectable and worthy granduncle, Dr. Maconchy, died : his successor and my uncle, George Maconchy, Esq., kindly offered me accommodation in his house, and many friends advised me to think of settling in Dublin, in the accoucheur line, by which branch of the profession Dr. Maconchy had realized a good fortune. Among my advisers on this occasion, the most strenuous was Dr. George Cleghorn, one of the oldest and most intimate of Dr. Maconchy's friends. From him I then, and often after, received very sound and useful lessons. No man knew Dublin better, and few could so readily direct a professional man as to the manner by which its inhabitants were to be pleased. When I decided on attempting a settlement in Dublin, my property was inconsiderable. From my father, I was entitled to between four and five hundred pounds. Most of this had been spent on education. A balance of one hundred and sixty-five pounds was the residue of my patrimony. To this I may add one hundred pounds, presented by the Rowley family on my return, with many compliments and professions of friendship. Of my allowance while abroad, I had not saved more than twenty pounds. On the 11th of July, 1781, I entered as pupil, in the Lying-in Hospital, and on the 28th of March 1783, was appointed Assistant to the Master (Dr. Rock), for both of which I paid him one hundred guineas, so that my cash was now very low. In June 1783, through the mediation of my friend, Counsellor John Dunn, I received and accepted a proposal to go abroad with John Jacob, Esq., of the county Tipperary. His health was bad, and his mother wished him to see a little of the Continent. My companion being in love with a Miss Gahan, to whom he was afterwards married, contrived to shorten his intended absence. We returned, in August 1784, to Ireland, and after visiting Killarney, and spending some weeks in Munster, I resumed my station of assistant in the Lying-in Hospital, on the 14th of September following. By this excursion, I acquired much valuable information, from visiting the London and Continental Hospitals, gratified a great deal of curiosity, diminished somewhat my youth (which is adverse to professional progress), and saved of my allowance two hundred and forty pounds. Once more, then, I felt myself independent. On the 11th of April 1785, I was admitted Licentiate of Physic in the College of Physicians, for which I paid thirty pounds, five shillings, and five pence. On the 7th of April 1786, I married Isabella Cleghorn, niece to Dr. George Cleghorn, with whom I got a fortune of fifteen hundred pounds, and on the 3rd of November following, was elected to the Mastership of the Lying-in Hospital.

"Such are the most material circumstances of my history that I can at present recollect ; they are thrown together in haste, yet they may, at some future period, serve to gratify the curiosity of some near relation, or assist my own recollection of past events. The registry of my practice will, in future, afford the best continuation of this sketch.

JOSEPH CLARKE."

"Dublin, December 1786."

The following particulars regarding Dr. Clarke's election to the Dublin Lying-in Hospital, show his abhorrence of the system by which GOVERNORS must be CANVASSED ; and are well calculated to point attention to the unwholesome ordeal through which medical appointments must be obtained : "When he was elected Master¹ of the Lying-in Hospital, there were three candidates for this very responsible appointment ; the preceding Master had died in office, one year before the completion of his term, which naturally gave rise to an active canvass. Dr. Clarke's previous strenuous exertions to promote the healthy condition of this institution, justly advanced his claims, and he was elected by a large majority. The following extract of a letter from his relative, Professor Cleghorn, dated July 1786, (Dr. Clarke had married his niece three months previously), is worthy of the writer, and well merits the consideration of all electors and candidates :—

¹ Master is the title of the physician who has medical charge of the institution, which office he cannot hold for more than a period of seven years, as regulated by charter.

'My dear Clarke,—I received your letter, requesting one from me to Dr. Halliday. My stomach revolts against the usual mode of extracting promises, and engaging votes before the governors can be sufficiently apprized of the merits of the candidates. It is founded on a supposition, that all men are actuated by selfish motives, regardless of the public good, and that they never consider whether their friend be fit for the place he wishes for, provided the place be fit for him. If you gain the election, I hope it will be by means fair and honourable; I would rather hear you had lost it, than that any others had been employed. The more a good character is enquired into, it will be so much the better for him who owns it; you must, therefore, be a gainer by standing the election, even should you fail of success, provided you are not too anxious about the matter, and suffer your mind to be too much dejected by a disappointment which could not have happened had merit been regarded, and which, after all, may probably tend more to your advantage than success would have done. Read the Tenth Satire of Juvenal, and reflect on the vanity of human fears and wishes—

"*Evertère domos totas optantibus ipsis
Dī faciles. Nocitura togā, nocitura petuntur
Militiā, etc.*

And then rest satisfied with having done your best to deserve success, though you have not obtained it.

"*Permites ipsis expendere numinibus, quid
Conveniat nobis, rebusque sit utile nostris.*

Carior est illis homo, quam sibi; nos animorum?

*Conjugium petimus, partumque uxoris; at illis
Notum, qui pueri, qualisque futura sit uxor.'*

"Mr. Rowley was on a visit to Lord Bective a day or two ago: I know not if he have yet returned home. I am just on the point of going to Somerhill.¹ It is probable I may write from thence, if I hear of anything interesting. Farewell, and believe me ever yours, etc.,

"GEORGE CLEGHORN." (pp. 22-4.)

The personal details contained in the concluding pages of the memoir are so interesting and so pleasant, that we transcribe them without abridgment. "It remains for me briefly to notice a few circumstances connected with the professional pursuits and private life of Dr. Clarke, from the period at which (1786), in the preceding pages, his early history concludes, up to the date of his death, in 1834, extending over a series of years, lengthened out far beyond the lot of most men, and characterized by uninterrupted prosperity, with high professional elevation, and the regard and esteem of his medical brethren.

"His income from practice rapidly increased, until it reached £3,000 yearly, from which amount for many years it varied but little. By accumulation, etc., his receipts, within the first twenty years, approached £5,000 annually, and this solely by his own exertions.

"In the early period of his life he was a tall, slightly-made man, and the fatigue of so laborious a profession was occasionally too great for his bodily strength; so much so, that, at times when returning to his home at night, after considerable exertion, he has told me, that if he walked a short distance, he was obliged to rest himself on the way from pure exhaustion. On one of these occasions of debility, he sent for Dr. Harvey (a well-known physician in Dublin) to advise what he should do; and after detailing to him his inability for so much work, etc., etc., Doctor Harvey asked abruptly, 'What

¹ "The seat of Lord Langford, near which Dr. Cleghorn's estate was, and where he now chiefly resided. The Rowley family, with one of the members of which Dr. Clarke had been on the Continent, were his zealous and warm supporters at the Hospital election.

do you eat for dinner?' and 'what do you take after it?' 'Indeed,' replied the doctor (whose appetite was but indifferent), 'I sometimes take a little mutton broth, with a little boiled mutton, and at other times chicken. I take very little wine, or occasionally a little drop of spirit in water.' Dr. Harvey replied, 'Phew—nonsense, man; take your roast mutton or your roast beef, with a pint of good port wine daily, and you will be as stout a man as any in the kingdom'. 'I took his advice', said the doctor; 'I speedily improved, and enjoyed excellent health afterwards.' He certainly did not take so much as Dr. Harvey's allowance of wine, but he took it much more freely than he had previously done. He was always a man of temperate habits; he took, I should say, as nearly as possible, half-a-pint of wine daily; this, from my own knowledge, I can state to have been his habit for the last twenty-five years of his life, during which I dined in his company almost daily, and for seventeen years of which I enjoyed the utmost happiness under his roof. His practice was almost entirely confined to the upper ranks of society; and, consequently, he got large fees, as may be inferred from the statement, that in fees varying from £10 up to £150, he received £37,252.

"In his fee-book, Nov. 11, 1801, where "£1 (note)" is entered, he adds, 'First of these vile productions.' The old gold guinea, of the value of £1 2s. 9d, Irish currency, was the usual fee previously, so that the innovation was far from agreeable to physicians.

"He invariably attended to his business with extreme regularity, so as to avoid hurry and confusion. His breakfast hour was nine o'clock, and dinner hour, five, which he observed with great punctuality, when not unavoidably obliged to be absent. He usually left his house in the morning at ten, or half-past ten o'clock, and from that hour, till half-past four or five o'clock, he was occupied visiting his patients. He seldom kept more than two horses for his own carriage, and used them daily, unless sickness, which rarely occurred, made it necessary to take one of the family horses. Several of his horses ran in this way for twelve or fifteen years, working regularly from five to six or seven hours daily. He frequently told me, that the true way to keep horses in health, was to give them plenty of work, and feed them well. They were always in good condition, and fully equal to their task. He never took them out of the stable after dinner, nor did he drive them any distance from town. For early and late visits, as also for country excursions, he used job or hackney carriages.

"Prosperous and exalted as was Dr. Clarke's professional career, from the commencement to its termination, it did not exceed his happiness in his domestic circle, until it pleased the Lord, on the 5th October, 1820, to take his only son, Dr. James Clarke, at the early age of thirty-two. His death was not alone the cause of deep sorrow to his father and all his relatives, but also of universal regret in this metropolis. It was occasioned by typhus fever, which he himself thought he had contracted in the discharge of his duty as physician to the Hardwicke Fever Hospital at the House of Industry. He had been on a short visit to his estate in the county of Fermanagh, where he had taken severe exercise; and on his return to town in a fatigued state, and labouring under the effects of a cold, he visited the hospital, and, when prescribing for a very bad case of fever, he received, as he said himself, a *whiff* from the patient, of the peculiar disagreeable consciousness of which he could not divest himself ever afterwards. He died on the twelfth day of the fever, throughout which I was one of those friends who anxiously watched over him. Few men were more universally beloved by their friends, and still fewer possessed, to the same extent, the esteem and regard of their professional brethren, which may be considered one of the truest tests of real worth. This was, indeed, a heavy affliction; but his Heavenly Father sanctified and blessed it, by making it the means of directing his powerful mind more earnestly towards his Saviour, there, from this divine and bound-

less source, to seek everlasting happiness, under the influence of the Holy Spirit. Subsequently, in 1829 and 1833, the Lord was pleased to take to Himself his only remaining children, two beloved daughters. It fails me to do more on this, to me, most painful topic, than to state, 'to them to live was Christ, and to die was gain'.

"In September 1834, Dr. Clarke left Dublin for Edinburgh, to be present at the meeting of the British Association, to which I have already alluded, as also to the object he had in view. He arrived there on Saturday afternoon, the 6th, and reached the house of his kind friend, Mr. Learmonth, of Moray-place, where he had been invited to remain during his visit. He had been very sick at sea, and after landing found his bowels in an uncomfortable state; he was, however, in excellent spirits, but appeared to his friends pulled down, either by the fatigue of the journey, or by sea-sickness. Some friends dined with Mr. Learmonth, and he partook of a variety of food, apparently with relish; he jokingly told Mr. L. he should take wine freely that day, as he found it peculiarly grateful to him. The conversation after dinner was animated, and he appeared to enjoy himself, and to anticipate great pleasure from the meeting of the Association. Sunday, the 7th, Dr. Clarke stayed at home, observing that he thought the repose might be serviceable. He passed the morning reading; at dinner, he ate as usual, and drank his wine cheerfully; he was animated, and talked a good deal, and was in excellent spirits, both at table and in the drawing-room. Monday, the 8th, the day appointed for the meeting of the British Association, he went out early in a carriage to procure his ticket for the public dinner, which was to take place that day, and afterwards called upon several friends. When the hour for dinner arrived, not feeling himself well, he said he was afraid of the heat that there probably would be in the room, and that he should prefer taking a little soup quietly at home, thus to be better prepared to encounter the evening meeting of the Association. Mr. Learmonth and our kind friend, Mr. Isaac Weld, who also visited Edinburgh to attend this meeting, left the dinner early, in order to call upon Dr. Clarke, and accompany him to the evening meeting. On their return, they found his stomach had been sick, and rejected its contents. He left the drawing-room, and requested a sofa might be placed in his bed-room, to which he retired. His pulse at this time was feeble, but not quick. He now told Mr. Weld he felt differently under this attack from what he had ever experienced before, and should it prove fatal, he requested his remains should be put into a leaden coffin, and taken to Money-more, where his father was buried, as he had always a great aversion to being interred in the crowded burying-ground of a city. He told Mr. Learmonth he had no fear of death, as his trust was fixed on his blessed Redeemer. When he was settled in his bed-room, and had some wine-whey, which he wished for, Mr. Learmonth and Mr. Weld left him, and as they considered him very ill, agreed to have medical advice instantly. Dr. Clarke had himself consulted Mr. Bell in the morning. As speedily as Dr. Abercrombie and Mr. Bell could be found, they visited him, which was about ten o'clock, p.m. They considered him dangerously ill. September 9th, he passed a very uncomfortable night, and suffered considerably from griping pain and spasm in the bowels. He asked to see his friend, Dr. Jackson of Dublin, who was then in Edinburgh, and gave him the paper which he had intended to read at the meeting of the medical section of the Association. Drs. Abercrombie, Jackson, and Bell afterwards met; they then gave over all hopes, and thought he could scarcely live to next morning. He was at this time in a torpid state, his voice thick and indistinct, and his features greatly changed.

"Wednesday, September 10; he continued in the same state of torpor in which he had been the previous evening, but his medical attendants found his strength had sunk greatly, and that his life was drawing fast to a close.

"He breathed his last at a quarter before one o'clock.

"In accordance with his expressed wish, the body was transmitted to

Money more. Much alarm was excited, as the disease was considered to have been cholera, which had for some time been prevalent. The morning he left home, while I was assisting him in the necessary preparations for his journey, he told me his bowels were rather relaxed, "but," he added, "you know this has always been useful to me." My reply was, that in such times I hoped he would be careful.

"The first intimation received in Dublin of his illness, was by a letter from our friend Mr. Learmonth to me, on receipt of which I instantly started for Edinburgh, and on my arrival, found, with heartfelt sorrow, that his remains had been forwarded to Glasgow the same morning, as it was not considered prudent to delay, owing to the excitement that prevailed.

"Such is a brief sketch of the last illness and death of my beloved uncle. Of him it may indeed be said, that as a physician or as a friend, he was without guile. Many, both in and out of the profession, when in difficulties, sought his counsel, impressed with the conviction, that he could and would give them sound advice to guide them safely through; nor did they ever find themselves disappointed.

"He was a man, naturally, of a most cheerful disposition, and his fine, expressive countenance so beamed with benignity, as to make the most favourable impression, even upon those who were strangers to him.

"I could, indeed, write a lengthened history of his genuine benevolence, and other Christian virtues; but from one to whom his unbounded kindness knew no limit, such would hardly be appropriate.

"His widow, who had ever been the most attached and devoted of wives, as well as the fondest and most exemplary of mothers, to my great satisfaction, resided after his death in my house. She died in her eighty-eighth year, having survived her husband thirteen years.

"To both, my thoughts ever recur with affection and gratitude, while I contemplate the memorable and happy events of which they were the source." (pp. 80-88.)

Thirteen years ago, Dr. Collins published his celebrated and invaluable statistics of 16,654 births occurring in his hospital practice: Dr. Clarke had previously given an account of 10,387 cases in his public practice; and in the volume before us, we have 3,878 cases which occurred in the private practice of Dr. Clarke, among the higher classes of Irish society, making in all a total of 30,919 births.

DIMINUTION IN INFANT MORTALITY IN THE DUBLIN LYING-IN HOSPITAL, IN CONSEQUENCE OF IMPROVED VENTILATION. "From the foundation of the hospital, in 1757, up to the period at which Dr. Clarke urged upon the attention of the governors the importance of the measures he advocated in 1783, of 17,650 infants born alive, 2,944 had died of convulsions, or, what is commonly termed, nine-day fits, as occurring within the first nine days after birth; thus, for the first twenty-five years the Institution was open, nearly every sixth child died. Such was the frightful mortality among infants in our Lying-in Hospital; and such it was which first prompted Dr. Clarke to examine minutely the mortality in similar institutions visited by him, in England and on the Continent, in 1783, from which he inferred that there must be something defective in the construction of the Dublin Lying-in Hospital, giving rise to a mortality found to be so immensely beyond that met with elsewhere; and thus, instead of forwarding the original design of Dr. Moss, the truly benevolent founder of the charity, of contributing to the preservation of young lives, producing quite a contrary effect.

"Dr. Clarke, in the communication which he had the honour of submitting to the governors, through the then consulting physician, Dr. Hutchinson, dated Brussels, 1783, declared his belief to be, that it was an accumulation of foul air, arising from an imperfect ventilation in the wards of the hospital, which proved so destructive of infantile health, and urged upon their atten-

tion certain measures for the removal, as far as possible, of the vitiated atmosphere. The following letter was received by Dr. Clarke at Bourdeaux, in 1783, in reply from Dr. Hutchinson.

"My dear Sir,—I have thus long delayed acknowledging your very obliging and acceptable letter from Brussels, that I might inform you about the steps we are taking in consequence of your remarks. Dr. Rock (the then master), Mr. Croker King (the consulting surgeon), and I, have had several meetings, in order to have some digested plan to lay before the board, as deliberations there are not generally the most advantageous to the despatch of business; nor do we think it prudent to divulge too publicly the disagreeable fact to which our attention has been called. *We find that almost all the children who die, are carried off by what the women call nine-day fits, as always attacking the infants within nine days from their birth—that when once seized, all remedies hitherto tried have been found ineffectual.* Your observations have been maturely considered, and, in consequence of them, we shall recommend the apertures in the ceilings, etc. etc. [After stating the means to be adopted to improve the ventilation, he concludes:] You may depend upon it, that (as far as in my power) due attention shall be paid to your remarks, and that the governors shall be informed to whom this humane inquiry is owing. I am, dear Sir, yours very sincerely,

"To Joseph Clarke, M.D., Bourdeaux.

F. HUTCHINSON.

"The recommendation of Dr. Clarke was immediately carried into effect, by the adoption of measures which provided for a free and easy passage of fresh air at all times through the wards, and which were executed in such a manner as not to leave it in the power of nurse-tenders or patients to control; the number of beds, also, in the large wards was reduced, and several changes were made in their construction, which rendered them more airy, and more easily kept clean.¹ The consequences of this improvement in the ventilation were (as reported by Dr. Clarke to the Royal Irish Academy² six years subsequently) immediate and most favourable, far surpassing the expectations of all concerned. Of *eight thousand and thirty-three* children, born subsequently to the wards being ventilated, only 419 died, that is about one in 19½, or from five to six in the hundred, instead of the enormous mortality before mentioned. Thus, by his skill and valuable suggestions, above 20,000 lives (humanly speaking) *have been saved*; as, had the mortality of infants continued *one* in six to this day, of the 158,436 (which is the total number born alive up to the end of 1847), we should have had 26,406 deaths, instead of 5,704, as the hospital registry now shows. This is an astounding fact, sufficient to arouse the attention of all those engaged in the management of large institutions, in which numbers of young creatures are assembled together under the same roof, and also well calculated to enforce upon them the necessity of paying that attention to *ventilation*, to which it is so preeminently entitled. From the instant the ventilation was improved, up to the present hour, *increasing* and incalculable benefit has been the result; and in proportion as Dr. Clarke's suggestions have been more rigidly carried into effect by the introduction of additional³ measures to free the wards from *impure* air, this most fatal disease has become more nearly banished, as is strikingly demonstrated by the results of the hospital registry for the *last twenty-five years*, compared with the *first twenty-five years* it was open before the ventilation was sufficiently attended to.

"The total number of children born from 1823 to 1847, both inclusive, was 54,074: of these only 501 died, being in the proportion of *one* in 108 nearly, in place of *one* in six, as in the first twenty-five years. We may

¹ See Dublin Medical Journal, vol. ix, 1836. Article on "Trismus Nascentium", by Dr. Collins.

² See the third volume of their Transactions.

³ See page 515 of my Practical Treatise for Additional Measures, adopted in 1829.

further observe, that had the mortality from the period at which the hospital was opened, been in the proportion of one in *one hundred and eight nearly*, as during the *last* twenty-five years, the deaths would have amounted to little more than *one-fourth*, even of the reduced number of 5,704, as now recorded in the registry; and thus the number of lives saved would have been 24,939, instead of 20,000." (pp. 18-22.)

THE SUCCESS OF DR. CLARKE'S PRACTICE is, we believe, unprecedented. In one case only did he use the forceps! Of 3,147 patients attended in a period of nearly 47 years, only 22 died. Of the 22 deaths, only 3 died from peritonitis, and in some of them were the symptoms—those of the low, typhoid form of puerperal fever, met with in the hospital at different times. Five patients died who were confined prematurely, one of whom was in an advanced stage of phthisis. Two died from the immediate effects of hæmorrhage. In two cases of shoulder presentation, in which turning was performed, the patients died. The ten remaining fatal cases do not admit of classification: and are thus described. They lose much of their interest, from the want of an account of the post-mortem appearances:—

"Mrs. A.—Presentation of shoulder; the child was turned without difficulty. This patient had a good recovery for the first fourteen days; she died, however, on the twenty-first day from her confinement. During the seven days preceding her death, she had three attacks of uterine hæmorrhage to a considerable extent. The child was slightly putrid.

"Mrs. B.—Shoulder presentation. This was a relaxed elderly lady. The child was turned with much difficulty. She died in a few days afterwards.

"The following cases do not admit of classification, and are, therefore, given separately:—

"Mrs. A.—This was a twin case (both girls). The head of the first child (which was putrid) was lessened. The second presented with the feet, and was born alive. There was not any difficulty in the delivery, nor had the labour been protracted. She died on the eighth day. Extremely foetid serous discharges took place from the uterus, and the pulse continued rapid and small from the time of delivery.

"Mrs. B. was confined after a labour of eight hours. She was attacked with pain in the region of the left kidney a fortnight afterwards, and died hectic at the end of a month from delivery.

"Mrs. C. was delivered of a male child, without difficulty in the labour. On the sixth day she had a paralytic stroke, and died convulsed on the tenth day.

"Mrs. D. was attacked with milk fever on the third day after delivery; had a second shivering on the fourth, and died on the thirteenth day. She had a strong tendency to mania from the time of her confinement, and was very despondent. The child presented with the breech; born alive; no difficulty.

"Mrs. E. was delivered of a boy; labour natural; was seized with pyrexia on the third day, ushering in scarlatina. She died on the fifth day. The patient was attended by Dr. Ivory, Dr. Clarke being at the time engaged with another. Three women died of scarlatina in the hospital the same year.

"Mrs. F. This lady had been greatly reduced previous to labour, by diarrhoea and grief. There was not any difficulty; the child, a male, was born alive. She expired in four hours after her confinement.

"Mrs. G. was confined without difficulty; the child, a male, alive. She had been labouring under dysentery for some time, which continuing unrelieved, terminated fatally one month subsequent to delivery.

"Mrs. H. This lady had a very easy labour. It was her first child, a girl, born alive. She died on the fourteenth day afterwards, of anomalous disease, most like bilious fever. She had been an invalid from birth; and laboured under a polypus in the rectum.

"Mrs. I. This patient died from laceration of the vagina. The child's head was hydrocephalic, and measured eighteen inches in circumference. The labour had not been tedious, but the uterine action was very strong. She died in fifty-one hours after her confinement.

"Mrs. K. This patient also died from laceration of the vagina; the labour was not protracted; the pelvis was defective." (pp. 43-4)

ARM PRESENTATION occurred nine times in 3,816 single births. Of these four were premature labours—"three at the sixth month and one at the seventh. Three of the four were putrid. In two of the nine, the foot came down with the arm; five of the nine children were born alive; one of the nine was still-born, but not putrid—in this case there had been considerable hæmorrhage before delivery; six of the nine children were turned. Of the three premature cases at the sixth month, in the two of them that were putrid, the breech was brought down without introducing the hand into the uterus: in one of these the arm was removed, and the breech brought down by the crotchet; in the other, the uterine action was strong, and, by gentle assistance with the crotchet, it was got down. Dr. Clarke was the *first* practitioner to recommend this mode of delivery, in preference to version, when the child is putrid; the safety of the mother is thus much less endangered.¹

"In the third case, at the sixth month, where the child was alive, the mode of delivery is not noted. Of the nine cases of arm presentation, all the mothers recovered. Two additional cases, in which the shoulder presented, and the children were turned, are already recorded in the abstract of the deaths of mothers. The proportion of presentation of the shoulder or arm, it may be observed, is one in 347 in Dr. Clarke's private practice, whereas in hospital we had one in 410.

"PRESENTATION OF THE FEET. In single births, there were thirty-six instances in which the feet of the child presented, which is in the proportion of one in one hundred and six (in hospital I had one in one hundred and twenty-nine). Of these, ten were boys, twenty girls, and in six the sex was not noted. Twenty-eight of the thirty-six were born alive—viz., eight boys, fifteen girls, and five in whom the sex was not noted. Two of the thirty-six were still-born, but not putrid; one, a boy, a *first* child, the other a girl, where there was great want of expelling power in the uterus, requiring the manual removal of the placenta also. Six of the children were born putrid—viz., one boy, four girls, and one where the sex was not stated. Of the thirty-six children, *twelve* were *premature* births, of which six were born alive and six putrid. Of the six born alive, one was at the eighth, two at the seventh, and three at the sixth month; of the six putrid, two were at the seventh, and four at the sixth month. In one case, where the child was born alive at the sixth month, the liquor amnii had been discharged three days previously. In one case, at the full period, it was complicated with placenta presentation, and in another with the funis.

"PRESENTATION OF THE BREECH. In forty-nine cases the breech presented, which is in the proportion of one in seventy-eight of the single births. In hospital, we had one in sixty-eight. Of the forty-nine children, *sixteen* were still-born, seven of which were *putrid*. Seven of the sixteen were boys, four of which were *putrid*; nine were girls, three of which were *putrid*. Twelve of the forty-nine were *premature* births, of which seven were born *alive*—viz., four at the seventh month, and three at the eighth. Of these seven, three were girls. Of the five premature still-born children, two were at the sixth and three at the seventh month. Four of the five were putrid, and three of the five were boys. This leaves eleven of the sixteen still-born children at the full period of gestation; of these, four were boys, one of which was

¹ See Dr. Collins's Practical Treatise, p. 71.

putrid. Of the three not putrid, one was a first child, and of large size. There was not any obvious cause to assign for the state of the other two. Of the seven girls, two were putrid. In two, the head was hydrocephalic; with one the mother had convulsions. Of the two remaining, in one the head was got away with great difficulty, and in the other there was no obvious cause.

"PLACENTA PRESENTATION. The placenta presented in nine cases, in each of which the hand was introduced, and the child turned, except in one instance, where, on the introduction of the hand, the feet were found lying above the placenta. Seven of the nine children were born alive. Of the two still-born, one was at the full period of gestation, and the other at the eighth month. There was not any difficulty in the delivery of either. Both the still-born children were males; and of the entire number, six were males, one was female, and in two the sex was not noted. In the case in which the feet presented with the placenta, there had been occasional hæmorrhage for some weeks before delivery; child alive. In a second case, where delivery was effected at the end of the eighth month, there had been occasional hæmorrhage for seven weeks previously; child alive. In a third case, the hæmorrhage had also been severe; child alive. In a fourth case, already recorded in the abstract of deaths, there was considerable hæmorrhage for three weeks before delivery; child alive. The second instance of death under this head, in which there was found a total separation of the placenta, and no opportunity for treatment afforded, can hardly be classified with ordinary cases. All the other mothers recovered well.

"Nine cases of presentation of the placenta, out of 3,847 deliveries, is a very large proportion. In hospital, we had *eleven* in 16,414; and Dr. Clarke, in his hospital report, gives four in 10,387. Thus it will be observed, that in *hospital* the proportion is one in 1,492, whereas in *private* practice the proportion is one in 427: and this we should anticipate, it being notorious that quietness is essential to patients labouring under hæmorrhage, the apprehension of danger from removal to hospital causing many to remain in their homes.

"ACCIDENTAL HÆMORRHAGE. Four cases of accidental hæmorrhage occurred. In three, on the membranes being ruptured, the discharge was checked, and labour in a short time succeeded. In the fourth case, alarming hæmorrhage came on at the end of the eighth month, in consequence of fright from a thunder storm, and the child was turned. Three of the four children were born alive. In one of the cases where the membranes had been ruptured, the child was still-born. Three of the four were premature births at the eighth month. All the mothers recovered.

"The proportion of accidental hæmorrhage is also considerably greater in private than in hospital practice—in the former being *one* in 962, and in the latter, one in 1,262.

"HÆMORRHAGE PREVIOUS OR SUBSEQUENT TO THE DELIVERY OF THE PLACENTA: ALSO RETENTION OF THE PLACENTA. In the following section are included cases of hæmorrhage occurring between the birth of the child and delivery of the placenta; also cases of retention of the placenta, requiring manual interference, in some instances accompanied with hæmorrhage, in others not; and in addition, cases of hæmorrhage subsequent to the removal of the afterbirth.

"There were twenty-five cases of retention of the placenta: seven of these occurred to the *same individual*, who, out of ten deliveries, in seven required manual assistance, for the removal of the afterbirth. In two of these there was hæmorrhage. In another instance, it was met with twice in the same patient. Of the twenty-five cases, the retention was accompanied by hæmorrhage in seven—one of which was a premature labour, at the sixth month, wherein, at the expiration of two and a half hours, the afterbirth was got away with the finger; in another, the feet of the child presented: this was the only preternatural presentation, where the placenta was retained. In *one* of the twenty-five, the child's head had been lessened, and in *two* the children

were born putrid. All the labours, with the exception of the one noticed, were at the full period of gestation. In *seven* of the twenty-five, the retention was owing to the irregular or hour-glass contraction of the uterus; in one of these there was hæmorrhage, and in another the child's head had been lessened, as already stated. In the eighteen instances where the placenta was retained without hæmorrhage, the hand was introduced, in almost all, at the expiration of an hour and a half subsequent to the birth of the child. All the mothers recovered favourably.

"With regard to hæmorrhage subsequent to the expulsion of the afterbirth, there was not any death from this occurrence. A few cases are noted where the loss sustained produced exhaustion and faintishness, for two, three, or four hours, but all yielded eventually to treatment—nor is it stated to have proceeded, in any instance, to a very alarming extent. The occurrence of hæmorrhage at this period, so as to require particular observation, was rarely met with.

"That Dr. Clarke, out of so vast a number of deliveries, should not have experienced any fatal result from retention of the placenta, or from hæmorrhage, either previous or subsequent to its expulsion, is a fact which cannot fail to impress the profession with a conviction of his profound practical knowledge. We doubt not that the diligent application of those principles of sound instruction, for ensuring the perfect contraction of the uterus, first inculcated by him, 'by pursuing, with a hand on the abdomen, the fundus uteri in its contraction, until the fœtus be entirely expelled, and afterwards continuing for some time this pressure, to keep it, if possible, in a contracted state', contributed largely to this favourable result. We know of no preventive of such importance in obviating retention of the placenta or hæmorrhage, previous or subsequent to its expulsion. The safety, too, and speedy recovery of the patient is intimately connected with the due contraction of the womb after the child has been expelled.

"TWIN BIRTHS. Thirty-one twin births were met with, or in the proportion of *one* in 124; whereas in hospital our proportion was *one* in 68, which is nearly one-half greater. Of the 31 cases *twelve* gave birth to two boys—*twelve* to two girls—*six* to a boy and girl, and there was one at the sixth month where the sex was not noted. In *nine* instances both children presented naturally; in *nine* one presented naturally and the other with the feet; in *five*, one presented naturally and the second with the breech; in *three*, the first presented naturally and the second preternaturally, but the exact presentation is not stated; in *one*, at the sixth month, the presentation is not noted, and in another the children were expelled before Dr. Clarke arrived. In *one* instance *both* children were still-born, one of which was putrid. In two other instances, the *second* child was still-born; in another, the first child was putrid and had the head lessened, while the second child was born alive; this was the only case in which instrumental delivery was required, as also was it the only case followed by fatal results to the mother: the labour was not severe, nor was there any difficulty in the delivery (see particulars in record of deaths). In twenty-six of the 31 twin cases both children were born alive, leaving the proportion of still-born *one* in 12—in hospital it was one in eight, or one-third more. In *five* of the 31 cases the labour was *premature*, viz., two at the sixth month, and three at the eighth; this is in the proportion of one in six—in hospital it was one in nine. Of the five premature labours, in two both children presented with the feet, viz.—one at the sixth, and *one* at the eighth month; in one at the eighth month, both were natural presentations; in another at the eighth month, the first was a natural presentation and the second preternatural; in the remaining one, at the sixth month, the presentation was not noted. In three instances, the placenta of the child first born was expelled before the birth of the second; the children were born alive.

"PRESENTATION OF THE FACE, AND OF THE FACE TO THE PUBES. Forty instances occurred, in seven of which the face presented, and in thirty-three the face was turned towards the pubes. Five of the children were still-born, four of which presented with the face to the pubes; one was, at the seventh month, putrid; in a second, the funis prolapsed, the pelvis was defective; in a third, no cause could be assigned, as there was not any difficulty in the labour; in the fourth, the labour was protracted, the patient very feeble, and the uterine action quite inadequate to the expulsion; the forceps was tried but without effect, after which the head was lessened. In the *fifth* case the face presented, the pelvis was undersized. This was the *same* patient already noticed, where the funis prolapsed in a previous labour with the face to the pubes. On the present occasion the head was lessened, after sixty hours labour. No other difficulty was experienced; the labour in almost all was easy and of short duration.

"PROLAPSED FUNIS. There were six cases in which the funis prolapsed; three of the children were still-born, one in which the face was turned towards the pubes, and is noticed under that head, where the pelvis was defective; a second in which the labour was severe, and continued for sixteen hours; and a third in which the frontal and parietal bones were absent. In three instances the head presented—in one the head with the hand—in one the feet, and in one the head with the face to the pubes. Four of the children were girls; one a boy, and in the defective child the sex was not noted.

"CONVULSIONS. Two cases of convulsions occurred; in one the patient had *three* fits previous to the birth of the child; the breech presented, and it was still-born. In the second case the patient had but one fit and that subsequent to delivery; the feet presented; it was born alive. Both mothers recovered favourably. It is a very singular fact, that the only cases met with should have been preternatural presentations. Only *one* case of convulsions occurred in the hospital, where the child presented preternaturally, during twenty-one years, in which term 48,379 women were delivered.—See Dr. Collins' Practical Treatise, page 200.

"LABORIOUS AND PROTRACTED LABOURS. There were eighteen cases of laborious labour, in twelve of which the child's head was lessened; in two of these the pelvis was very defective, under three inches; in one the chin was to the sacrum, and although the uterine action was very powerful, no progress was made; two cases are recorded under face presentations with defective pelvis; in the sixth the labour was protracted to ninety hours; in the seventh to eighty; in the eighth, delivery was effected at the end of thirty-six hours severe labour; in the ninth at the end of forty hours; in the tenth, after twenty-four hours from the dilatation of the os uteri, the head was hydrocephalic, and in two the duration of labour is not noted. In these cases Dr. Clarke had assistance from professional friends. One of the eighteen was delivered with the lever; child still-born. In the remaining *five* cases of very severe and protracted labour, instrumental delivery was not had recourse to; one was seventy-two hours in labour—child alive; one forty hours severe labour—child alive; one thirty-six hours after full dilatation of the os uteri—child alive; one seventy-two hours—child still-born, it was very large, and the period of gestation had been protracted seven days beyond the nine months; one forty-eight hours—child still-born. All the mothers recovered favourably. Some additional cases of tedious labour are noted, but not of such a nature as to require any remark.

"Such is a brief enumeration of the instrumental deliveries in Dr. Clarke's practice, extending over the long period of *forty-four* years, including *three thousand, eight hundred, and seventy-eight* births. This is in the proportion of one in 298, so that it is obvious he did not consider a frequent resort to instruments, in order to effect the *hasty* delivery of the patient, necessary to ensure safety, and that necessity alone induced him to use them. It may be observed he only used the forceps once, and that without completing the

delivery. If we seriously reflect upon the happy results to the *mothers*, from the practice pursued by this distinguished physician, as regards the use of instruments, and then carefully examine the succeeding section upon children *still-born*, and find here equally happy results (as of the 3,816 *single* births, there were only *forty-two* children still-born of those that had arrived at the full period of gestation, or in the singularly small proportion of *one* in 91), we cannot fail to discover a number of *astounding* truths, sufficient to warn our *artificial* advocates, and make them pause until *they can supply* their professional brethren with a series of facts equally satisfactory.

"When Dr. Clarke commenced practice in Dublin, it is a well-known fact (which my friend Dr. Labatt, who is fully acquainted with the circumstances, clearly substantiates), that almost every accoucheur then, in any amount of practice, invariably carried the lever, or forceps, in a pocket expressly made for the purpose, so as to have either instrument in readiness for use in every case they were called upon to attend, so necessary then were these instruments generally considered.

"We doubt that any physician ever attended a greater number of patients in a similar rank of life, which arises from the lengthened period during which Dr. Clarke was enabled to pursue his professional duties, as also the active manner in which he was engaged. When we look to the truly favourable results, we require no other proof to convince us of the profound skill which enabled him to conduct so vast a number of cases to so happy a termination. The deaths were in the proportion of *one* in *one hundred and seventy-five* deliveries; and when we examine into the cause of death in the twenty-two cases recorded, we find that there were *three* from peritonitis, *five* from phthisis, *one* from hectic fever, *one* from scarlatina, *one* from a paralytic stroke, *one* from diarrhoea, *one* from dysentery, and *one* from an anomalous disease, thus leaving *eight* deaths as the results of childbirth, or in the proportion of *one* in 477.

"How seldom should most practitioners be found to use instruments, if the successful course pursued by Dr. Clarke were universally aimed at. Is it not worthy of our best consideration, with the invaluable statement before us, that, in an extent of practice in the upper ranks of life perhaps unexampled, there is *not one single instance of death* resulting from *laborious or protracted labour*. This is a practical fact, which ought to be carefully recollected, and seriously weighed, by most of our Continental brethren, who use instruments in every 5th, 10th, 15th, 20th, or 30th labour under their care, with the object of *expediting* delivery; as also by some of our own countrymen, whose unsound doctrines, inculcating mischievous interference to promote *hasty* delivery, the unquestionable truths here recorded clearly demonstrate to be unjustifiable, and most uncalled for. Should not this inexpressibly important record for ever silence those who venture to publish crude and fanciful opinions, unsupported by any data from their own experience affording similarly happy results. It affords me infinite satisfaction to supply this truthful registry of facts, for the universal and serious consideration of the profession. These truths speak in language the most convincing, and must, when studied, leave an indelible impression.¹

"**STILL-BORN AND PREMATURE CHILDREN.** The children still-born may be considered with advantage after the subject of laborious labours. Of the 3,816 single births, there were one hundred and twenty still-born, or in the proportion of *one* in thirty-two. Only *forty-two* of the one hundred and twenty were at the *full* period of gestation, nineteen of which were boys, and twenty-three girls. Of the nineteen boys, six were putrid. Of the twenty-three girls ten were putrid. Of the *forty-two* still-born at the full period, thirty-eight may be found recorded under the following heads, viz.,—laborious

¹ See Dr. Collins's Letters to Dr. Simpson, *Prov. Med. and Surg. Journal*, 1848: also, Dr. Simpson's reply in same Journal.

labours, convulsions, prolapsed funis, face presentation, breech, feet, and arm presentation, hæmorrhages, and mothers dead. Of the four remaining, one was caused by an upset in a carriage; one occurred where the labour was natural, and only of twenty-four hours' duration; and in two the head was hydrocephalic. *Seventy-eight* of the 120 were *premature* births, *sixty-five* of which were *putrid*, viz.—seven at the 5th month, twenty-two at the 6th, twenty-four at the 7th, and twelve at the 8th month. Of the *sixty-five putrid*, *thirty* were male, twenty-two female, and in thirteen the sex was not noted. *Thirteen* of the *seventy-eight* premature children were not putrid, viz.—four boys, five girls, and four in which the sex was not noted.

“Five children were still-born in the *twin-births*, all at the full period of gestation, which see under that head.

“In addition to the *seventy-eight* premature *still-born* children, there were *one hundred* premature children born alive, viz.—nineteen at the 6th month, thirty-seven at the 7th month, and forty-four at the 8th month; of these, fifty-six were boys, thirty-four girls, and ten in which the sex was not noted.

“Of the *total* number of *premature* children, both alive and still-born, viz., 178, *ninety* were males, *sixty-one* females, and in twenty-seven the sex was not noted. If we deduct the *premature* still-born from the *total* still-born, the proportion will be reduced two-thirds, or to one in ninety-one.

“The *proportion* of children born *prematurely* in the upper ranks in life, is much greater than that observed in hospital. This observation holds good also as regards children born in a *putrid* state. Of the 120 still-born, two-thirds were premature, and more than two-thirds of the 120 were *putrid*. In hospital we had about *one-fourth* premature, and rather less than *one-half* putrid. This fact powerfully proves the greater delicacy of constitution in the upper ranks of society, engendered by the mode of life pursued, coupled with other circumstances; as, notwithstanding the numerous casualties to which the lower orders are necessarily exposed in the laborious pursuits they are compelled to follow, a much greater portion of their offspring arrive at the full period of gestation in a healthy state. The practical lesson is obvious in the treatment of those who live in luxury, and often without any kind of healthful exercise.

“Of the *total* number of patients confined under Dr. Clarke's care, viz., 3,847, three thousand eight hundred and sixteen gave birth to *single* children, of which 1,949 were boys, and 1,840 girls; in twenty-seven the sex was not noted. Thirty-one gave birth to *twins*. The proportion of *male* to *female* children is not by any means so much greater in the upper ranks of life as amongst the lower classes. In hospital, the proportion is about twelve males to eleven females, whereas the above is as eighteen to seventeen nearly.”(pp.46-62.)

HISTORY OF THE CASE OF THE PRINCESS CHARLOTTE. The following letter from Dr. John Sims to Dr. Clarke gives an authentic narrative of this lamentable catastrophe. As all concerned are now removed from this world, there is no impropriety in Dr. Collins publishing the document; and there is no restraint on our saying, that the Princess might most probably have recovered, had her condition been early understood, and proper means been resorted to, to excite contraction of the uterus. She evidently died from hæmorrhage, a small quantity of blood being sufficient, in her exhausted state, to prove fatal:—

“London, November 15, 1817.”

“My dear Sir,—I do not wonder at your wishing to have a correct statement of the labour of Her Royal Highness Princess Charlotte, the fatal issue of which has involved the whole nation in distress. You must excuse my being very concise, as I have been, and am very much hurried. I take the opportunity of writing this in a lying-in chamber.

“Her Royal Highness's labour commenced by the discharge of the liquor amnii about seven o'clock on Monday evening, and pains followed soon after; they continued through the night and a great part of the next day, sharp,

short, but very ineffectual. Towards the evening, Sir Richard Croft began to suspect that the labour might not terminate without artificial assistance, and a message was dispatched for me. I arrived at two on Wednesday morning. The labour was now advancing more favourably, and both Dr. Baillie and myself concurred in the opinion that it would not be advisable to inform Her Royal Highness of my arrival. From this time to the end of the labour the progress was uniform, though very slow, the patient in good spirits, pulse calm, and there never was room to entertain a question about the use of instruments. About six in the afternoon, the discharges became of a green colour, which led to a suspicion that the child might be dead; still the giving assistance was quite out of the question, as the pains now became more effectual, and the labour proceeded regularly though slowly. The child was born, without artificial assistance, at nine o'clock in the evening. Attempts were for a good while made to reanimate it, by inflating the lungs, friction, hot bath, etc., but without effect; the heart could not be made to beat even once. Soon after the delivery, Sir Richard Croft discovered that the uterus was contracted in the middle, in the hour-glass form, and, as some hæmorrhage commenced, it was agreed that the placenta should be brought away by introducing the hand. This was done about half-an-hour after the delivery of the child, with more ease and less loss of blood than usual. Her Royal Highness continued well for about two hours; she then complained of being sick at stomach, and of noise in her ears; began to be talkative, and her pulse became frequent, but I understand she was very quiet after this, and her pulse calm. About half-past twelve o'clock she complained of severe pain at her chest, became extremely restless, with a rapid, irregular, and weak pulse. At this time I saw her for the first time, and saw immediately that she must die. It has been said we were all gone to bed, but that is not a fact. Croft did not leave the room, Dr. Baillie retired about eleven, and I went to my bed-chamber, and laid down in my clothes at twelve. By dissection, some bloody fluid (two ounces) was found in the pericardium, supposed to be thrown out in articulo mortis. The brain and other organs were all sound, except the right ovary, which was distended into a cyst, the size of a hen's egg; the hour-glass contraction of the uterus was still visible; a considerable quantity of blood in the cavity of the uterus, but those present differ about the quantity, so much as from twelve ounces to a pound and a half; the uterus extended as high as the navel. The cause of Her Royal Highness's death is certainly somewhat obscure; the symptoms were such as attend death from hæmorrhage, but the loss of blood did not appear to be sufficient to account for a fatal issue. It is possible, that the effusion into the pericardium took place earlier than was supposed, and it does not seem to me to be quite certain that this might not be the cause. As far as I can judge, the labour could not have been better managed. That I did not see Her Royal Highness more early, is awkward; and it would have been better that I should have been introduced before the labour was expected; and it should have been understood, that when the labour came on I should be sent to, without waiting to know whether a consultation was necessary or not. I thought so at the time, but I could not propose such an arrangement to Croft. But this is entirely *entre nous*.

"I am glad to hear that your son is well, and, with all my family, wish to be remembered to him. We were happy to hear that he was agreeably married. I remain, my dear Doctor, ever yours, most truly, JNO. SIMS."

"P.S. This letter is confidential, as perhaps I might be blamed for writing any particulars without the permission of Prince Leopold." (pp. 68-70)

Dr. Robert Lee has republished in the *Lancet*, 28th April, 1849, p. 451, as an addition to Dr. Sims' narrative, the following account, which first appeared three weeks after the event. He thinks that "there is internal evidence to prove that the information was communicated by Dr. Baillie, from the verbal report furnished to him by Sir Richard Croft."

"The editors having been sufficiently apprized that the profession expected from them some account of this case, the lamentable termination of which has cast such a settled gloom over the British empire, immediately on learning that the physicians who attended it did not mean to publish any statement (a resolution on the propriety of which, under the circumstances, they perfectly coincide), strenuously endeavoured to obtain every information respecting it from such sources as could be depended upon. Their exertions have been successful, and they are now enabled to present a report to their readers, which may be regarded as strictly authentic.

"The Princess Charlotte previous to her confinement was in good health, and immediately under the eye of her accoucheur, Sir R. Croft, who resided at Claremont for three weeks up to the moment in which she was taken ill. Dr. Baillie, also, was in attendance, chiefly, we have been informed, on account of a promise exacted from him by the Princess, that he would be near her on this occasion. Her spirits were excellent, and she anticipated only the most favourable issue of the event, which was hourly expected.

"She was first made sensible of her approaching delivery at seven o'clock on Monday evening, the 3rd of November; but the labour-pains were so inefficient, although acute, as scarcely to evacuate the water, which had ruptured the membranes at the commencement of the labour—a circumstance, however, which every accoucheur knows prognosticates nothing either uncommon or untoward. In this manner the labour proceeded slowly for twenty-six hours, the Princess being frequently up, and walking about, from finding that the pains almost left her when she was in the recumbent position. About this time, also, judging from the inefficiency of the pains, and the little progress made in the labour, we understand that Sir Richard Croft suspected that there were either twins, or that there existed some irregular action of the uterus; and as it was probable a consultation might ultimately be required, he wrote to Dr. John Sims, requesting his immediate attendance. He had in the meantime provided everything that could be wanted, should it be found expedient to have recourse to artificial delivery.

"Dr. Sims arrived at Claremont at two o'clock in the morning of Wednesday, but did not then see the Princess; and as the cause of this has been grossly mis-stated, we think it proper, in justification of an honourable man, and so highly respected a member of the medical profession as Sir Richard Croft is well known to be, to state, that we have been informed, from a quarter which we must credit, that it was proposed by Sir Richard to Dr. Sims, that he should then be introduced to the Princess; but both Dr. Sims himself and Dr. Baillie thought his presence at that time could not be productive of any benefit, but might agitate the patient. Dr. Sims therefore declined entering the lying-in room. No consultation was at this period necessary, as the labour was evidently advancing, though slowly; but on hearing the statement of the situation of the Princess from Sir Richard Croft, Dr. Sims concurred in the opinion, that everything should be left to nature.

"About noon on Wednesday, it was first suspected that the child might be dead, or that it might be born in a state of suspended animation, and every known means of recovery were immediately prepared. Still the labour continued to be scarcely progressive, the pains being such as tend to forward birth rather by moulding the head, so as to admit of its easy passage, than by forcible expulsion. When this was completed, the pains became more efficient; and, at the termination of fifty hours from the commencement of the labour, the Princess was delivered, by natural efforts, of a still-born male child. No great discharge followed the birth, but it was soon discovered that the uterus was acting irregularly, and taking on the hour-glass contraction; and an unfavourable separation of the placenta was anticipated. This likewise, in some degree, accounted for the protracted character of the labour.

"At half-past nine o'clock, a discharge of blood occurred. Dr. Sims, who was then employed in an adjoining room in endeavours to reanimate the

infant, was instantly informed of this occurrence, and in consultation with Sir Richard Croft, agreed that the immediate separation and removal of the after-birth was necessary. It was effected with little difficulty, and was followed by a very trifling discharge, either of fluid or coagulated blood.

"The Princess was now as well and composed as ladies usually are immediately after delivery, and continued so until a quarter before twelve o'clock, taking frequently small supplies of nourishment; but at this time she became restless, and rather talkative, and complained of being sick. She vomited, but nothing was ejected, except a little camphor julep, which she had taken; and at this moment her pulse was firm, steady, and under 100; she again was composed. About half-past twelve, however, the breathing became impeded; the respiratory organs were evidently under the influence of spasm, and continued in that state till she breathed her last, at half-past two o'clock, exactly five hours and a half after her delivery.

"In this afflicting state of the case, Dr. Baillie and Dr. Sims, who had been called into the room when the breathing first became affected, united their judgment and skill with that of Sir R. Croft, but in vain, to avert the impending calamity. Art proved unavailing, although everything which it could devise, and which experience could suggest, was attempted.

"On the 7th of November, the body was opened by Sir Everard Home, assisted by Sir David Dundas, Mr. Brande, and the apothecary of Prince Leopold's household; and the following, we believe, is a pretty accurate statement of the appearances these gentlemen observed:—

"The membranes of the brain presented their natural aspect; the vessels of the pia mater were less distended with blood than was to be expected after so severe labour; the ventricles of the brain contained very little fluid; the plexus choroides was of a pale colour, and the substance of the brain had its natural texture. The pericardium contained two ounces of red-coloured fluid; the heart itself, and the lungs, were in a natural state; the stomach contained nearly three pints of liquid; the colon was distended with air; the kidneys and other abdominal viscera were in a natural state. The uterus contained a *considerable* quantity of blood, and extended as high up in the abdomen as the navel, and the hour-glass contraction was still very apparent." [*The London Medical Repository*, December 1, 1817, vol. viii, p. 534.]

PRACTICAL OBSERVATIONS ON THE PREVENTION, CAUSES, AND TREATMENT OF CURVATURE OF THE SPINE. With Engravings and Woodcuts, Illustrative of the Cases. By SAMUEL HARE, Surgeon. *Third Edition, revised and enlarged.* pp. 245. *Plates.* London: 1849.

MR. HARE'S practice seems both sensible and successful. It is, we may add, essentially such as is generally adopted in the curative and preventive treatment of Curvature of the Spine, and the consequences which arise from that deformity. The author has not attempted to give an air of novelty to a subject which did not admit of it. Mr. Hare would fain see our ladies so clothed as not to destroy their health and gracefulness; but we fear that the influence exerted by the despotic trade of female dressmakers is little likely in our day to succumb either to the requirements of health, or the perception of the fact that artificial shapes, even though invented in Paris, and retailed in the best London shops of the West End, are only miserable caricatures of the workmanship of the Almighty. Education, and, above all, the more general cultivation of the arts of painting and sculpture, are more likely to bring about the needed reformation, than medical writings; which, now a-days, alas! in the estimation of the public, rank but little, if at all, above the lucubrations of homœopaths, hydropaths, and hygeists, all of whom have their appointed periodicals duly published, and as regularly advertised in the *Times*. We are, therefore, of opinion that the diffusion of a taste for what is beautiful in art will be the most potent influence in dispelling the present delusions, as to there being anything but

what is repulsive in the pasteboard, pads, whalebone, and mohair, in which our women are encased. The following is Mr. Hare's discourse on stays.

"STAYS. The use of the zone or girdle, the type of our modern stays, is of very ancient origin; and it is probable that in all ages of civilized life, the sex has used some article of this kind, from an idea that it was convenient for the support and graceful carriage of the figure. On their first employment, stays were of simple construction, and were destitute of their present objectionable properties, being resorted to, almost exclusively, for the purpose of suspending from them other articles of dress in any easy, flowing, and graceful manner; and whilst restricted to such uses, and not drawn unnecessarily tight, they would not be likely to be attended with any mischievous effect. It is probable that most of the errors and foibles of mankind have had their rise from some motive or notion, not culpable in itself, but deserving censure only from being carried to excess; thus, the unnatural construction and excessive compression of stays have led to an accumulation of bodily suffering and deformity, of the extent and consequences of which few were, or are, fully aware.

"There are, at the present time, thousands who, ignorant of the misery they are inconsiderately providing for themselves, are daily sacrificing health, and sometimes shortening life, to the mere vanity of desiring to possess what a vitiated taste calls "a fine figure." Our promenades, public streets, and places of fashionable resort, afford abundant evidence of the sad effects resulting from the universal prevalence of this baneful practice. The notion that a woman is more beautiful with a remarkably small waist, ought long ago to have been exploded;—as well might we admire as beauties, the flattened heads of some tribes of Indians, or the extremely contracted feet of the Chinese. Genuine taste admires no such eccentricities.

"That women should experience a feeling of support from the use of stays, after wearing them from early childhood, admits neither of doubt nor surprise; the only wonder is, that they should feel comfortable without them even during the hours of repose.

"A moment's consideration of the anatomy of the chest, will shew still more forcibly the injurious tendency of stays, as ordinarily made, for while the termination of the eighth, ninth, and tenth ribs in long and yielding cartilages, and the want of any attachment of the anterior extremities of the two remaining ribs, increase the mobility of the chest, it will at once be seen how readily, from this very circumstance, the circumference of the waist may be acted upon by the strings used in the dresses of children, or the stays worn by females during childhood or whilst the body is in a state of growth; indeed, it is obvious there is but little power in those parts to resist even a very moderate degree of pressure.

"But modern stays are constructed with so little attention to the form of the body, that the pressure is the greatest upon the lower part of the chest, which is naturally the widest, whilst there is the most room at the upper part, where its diameter is the smallest; thus, in effect, inverting the order of nature, and causing a complete transformation of this important portion of the body, by making its base uppermost, and its apex downwards; they are also made so long as to cause injurious pressure on the pelvis, their tendency being, therefore, to turn the crest of the ilium inwards.

"There are other and striking evils resulting from tight lacing; by the pressure of stays, the functions of the vital organs are injured, and the whole frame is impaired; the bones of the chest being contracted, and their natural extent of motion diminished, prevent the free action of the lungs; the blood, not being sufficiently decarbonized by respiration, becomes deteriorated in quality, and consequently the various systems of the body suffer either in structure or function: the countenance ceases to have its healthy aspect; energy and muscular action become impaired; while palpitation of the heart, a quick pulse, and difficult respiration, with a much diminished breathing

capacity of the chest, are usual symptoms where tight lacing has been long persevered in. The pressure and confinement of stays also produce great derangement of the functions of digestion, preventing the stomach from dilating in the proper direction on the reception of food, and also impeding the natural peristaltic motion of the intestines; and, in some extreme cases, entirely changing the form and position of the viscera, which are then pressed towards the lower part of the abdomen, and so compressed that their proper offices in the animal economy cannot be adequately performed. Hernia also, it is known, is sometimes induced by this improper pressure.

"As the stays scarcely allow of lateral motion, or indeed of any other with freedom, the muscles of the chest and back often become atrophied, and as the spine gradually gives way, the other bones of the chest become displaced from their natural position, the sternum being in some cases forced inwards, in others the reverse; the ribs, instead of having the graceful curve which they naturally possess, become—in the part below the axillæ—completely flattened, and their extremities, instead of being directed forward, project almost directly downwards, so that the conical form of the chest is, as has already been stated, inverted.

"The author has made repeated measurements with a view to compare the circumference of the waist and the width of the stays of a great number of females, and has uniformly found a great difference between the two,—the former often measuring from two to four inches more than the latter; and as the stays are unyielding, and yet made to meet closely behind, it is clear that the difference must be made up by the compression of the body. This is an experiment which all parents have it in their power to make, and the correctness of which they can ascertain.¹ But it should at the same time be remembered, that the difference between the circumference of the waist and the length of the stays, does not represent the entire amount to which the chest is compressed; because, by the habitual use of tight corsets, the ribs become more or less *permanently* contracted, and to this contraction no corresponding expansion occurs when the stays are unlaced. The difference indicated, is really the amount of the mobility remaining when the pressure is removed—not from the ribs in their natural condition, but from a chest already more or less injured, and permanently contracted by the use of the stays. Need we then be surprised that the female figure is so frequently and so lamentably deformed? Rather ought it to excite our astonishment that so many of those subject to the causes should escape the consequences.

"Some unfortunate sufferers, by placing soft pads in the lateral curve, frequently pass years without its being known that such distortion exists; but their lives, under these circumstances, must be spent in a state little short of misery, on account of the languor, debility, and mental as well as bodily suffering, which they endure.

"Not much less objectionable than stays, are the various instruments sometimes made use of to amend or protect the shape, which, so far from improving, they tend ultimately to injure; indeed, most of such inventions, instead of being useful in the prevention of the deformity, often accelerate its progress. If parents were acquainted with the natural state of the human body, so as to enable them to comprehend the objectionable nature of these and similar devices, they would reject the use of such restraints, and clothe their children in dresses, which would not press injuriously on any part of the frame.

"If tight lacing be attended with such mischievous effects to young females and women in general, the evil is still more increased when practised during the period of the pregnancy. Whoever attentively considers how much the future health of the offspring depends upon the mental and physical condition of the mother whilst in this state, must be convinced that all undue compression of the body is highly improper: it is as inimical to the welfare of the mother, as to that of the child, not only impeding the full develop-

¹ The measurement should be made in the morning before dressing.

ment of its members, but rendering its birth more dangerous in proportion as the circumference or diameter of the pelvis of the former is narrowed—modern stays, by extending so low as to embrace the hips, having a direct tendency to produce this effect. The uterus, as it increases in bulk, necessarily elevates the viscera, which, being forcibly pressed by stiff, unyielding stays, cause of course great inconvenience to the mother. Defective secretion of the milk, the food designed by nature for the infant, and so essential to its nourishment and preservation, may also arise from the same cause. The difficulty of breathing likewise, a frequent complaint with pregnant women, is greatly increased by the restraint in which the respiratory organs are placed by the additional restriction.

“Notwithstanding all that has been said and written on the evil effects of modern stays as they are usually worn, and although they have been proved, in the clearest manner, to be one of the most formidable causes of diseases of the spine (*See the chapter on Lat. Curv.*), yet an attempt to induce females altogether to discard so common an article of attire, would be exacting more than can perhaps be expected. What is to be hoped for is, that medical men, by showing the mischiefs attending their use in their present form, may induce the sex to make such alterations in their construction as will, in a great degree, obviate the attendant evils.

“The chief objects to be had in view, in constructing these articles so as to be innoxious are, to render the pressure on every part as slight as possible, in order that there may be the utmost freedom of motion; to admit of the greatest amount of pliability; and to ensure a proper adaptation to the figure. In growing girls, instead of the stays being tightly girt behind by laces, they should be secured in front by buttons or strings; in adults, there may be no objection to laces, provided the stays be, in other respects, of proper construction, and in this case the lacing should be in front, a strip of India-rubber webbing, about an inch in breadth, being inserted on each side the lace-holes, and a similar one, of double breadth, down the middle of the back; the gussets for the part of the stays covering the hips,¹ and for that supporting the breasts must be made of the same elastic material; the shoulder straps should pass directly over or upon the shoulders, and be so constructed as to lie flat upon them by being inserted obliquely into the stays. It is not necessary to enter into further details as to the material and construction of stays, except to observe that the fabric should be of a firm but not altogether unyielding nature, and that they should be constructed so as to allow free motion in every direction. The only whalebones required will be two thin ones to protect the lace-holes, and two, equally thin, on each side, to prevent the stays from puckering; by these means, if the stays are at all proportionate to the size of the body, active exercise, which is absolutely necessary for increasing the strength of growing girls, can be freely used, and tight lacing will be next to impossible.

“There are other portions of female dress which are injurious as regards their effect upon the health and form of the body; these are, the strings of petticoats, aprons, etc., which are generally drawn very tightly round the waist, thus contributing to the ill effects which have been previously detailed. Parents ought to be especially careful that the articles of dress be suspended by buttons and pins, and that strings should be, as much as possible, dispensed with, especially during the period of growth.

“A custom at present prevails to a very considerable extent, of using a leathern belt buckled round the waists of boys when they commence wearing their clothes of woollen cloth; this practice, unless adopted with great care, has a direct tendency to produce a contracted state of the chest and upper part of the abdomen, similar in effect, though not in degree, to that produced by corsets in growing girls; it is hoped that it is only necessary to point out the evil that it may be avoided.” (pp. 28-36.)

¹ It is still more proper that the stays should not embrace the hips at all.

CRITICAL DIGEST OF THE BRITISH AND FOREIGN MEDICAL JOURNALS.

ANATOMY AND PHYSIOLOGY.

ON THE ART OF MAKING TRANSPARENT PREPARATIONS OF THE SPINAL CORD, FOR SHOWING THE ROOTS OF THE NERVES BY THE MICROSCOPE.

By JOSEPH SWAN, F.R.S.

THE medullary matter of the spinal cord is formed of meshes, very similar to those of portions of the brain. The grey matter of each quarter of the cord radiates externally; the medullary matter intervenes between the rays and receives it into its meshes, and in this manner the two kinds become gradually combined. The radiation of the grey is best seen in a thin dry horizontal section, which has been allowed to dry exposed to the air, and afterwards made smooth and varnished. A fresh slice covered with thin glass does not answer, as its character becomes in a great degree lost by drying.

The roots of the nerves, just about their collection into fasciculi, appear on the surface of the spinal cord immediately underneath the pia mater as a layer of coarser wavy threads; from this part they pass rather obliquely through the medullary matter towards the grey in finer wavy threads, which communicate very much with each other, and with the meshes of the medullary matter, and the extension of these amongst the grey. In some of the preparations distinct white filaments are seen ramifying over veins which remain coloured by the blood they had contained. Every filament is accompanied by an artery; and when this is more prominent, there is a more harsh definition of the nerves. When the preparation has been made very transparent, the nerves disappear and become involved in the meshes of medullary matter; at the same time the arteries also become obscure. From the appearance of the preparations, it seems as if there probably existed a continuous layer of roots from one end of the spinal cord to the other. The wavy appearance of the roots on the surface and of those descending into the substance of the cord, may be supposed to resemble very distantly the fibrils of the roots of plants. Their character is peculiar, and different from any other arrangements of nervous matter, but by no means less beautiful.

The spinal cord receives blood not only by its anterior and posterior arteries, but by very very numerous minute vessels which accompany the fibrils of every nerve into its substance: these do not divide directly amongst the roots of the nerves, but, at one side or underneath each layer, give off branches which form somewhat circular brushes of capillaries, and these become insinuated amongst the roots of the nerves.

The pia mater and arachnoid membrane have both a cellular appearance, which may be mistaken for the meshes of medullary matter. This cellular appearance is obliterated by applying Canada balsam, but restored again by washing this off with spirits of turpentine: this effect is very convenient in making dry preparations of the nerves, as the membranes do not then much obstruct the demonstration of the minute roots underneath.

As the spinal cord (especially in large animals, as that of the ox, from which my preparations have been chiefly made) does not dry sufficiently transparent for exhibiting the roots of the nerves, I have been under the necessity of somewhat modifying the process, which I previously communicated, for making transparent preparations of the brain; and I will endeavour to describe the change required as briefly as possible.

The spinal cord is to be cut into pieces of one or two inches long, so that

each may include all the roots of one or more nerves in each quarter. The dura mater is to be removed, and the nerves preserved as far as the ganglia. Each portion of the cord is then to be divided through the median line, and each half again between the appearance of the anterior and posterior nerves, so that there will be four quarters separated. As the roots of the nerves enter rather obliquely, it is necessary to cut off, close to the nerve, a somewhat triangular portion of each side of one of the quarters, so as to make the preparation a flat piece, containing the nerves and their continuation through the medullary and grey matter. The piece thus cut is to be placed on a glass slide, and dried before the fire on a plate covered with paper. In two or three days it is to be raised from the slide with a thin scalpel, and the soft matter underneath is to be carefully removed ; it is then to be placed on a fresh slide, and gently pressed on this with the finger, and there remain until it is dry. When dry it is to be raised again from the slide, and turned over, that any matter preventing the transparency may be removed : this is known by holding it from time to time to the light. This process of clearing is facilitated, by moistening it with spirits of turpentine now and then dropped on it. If it had become uneven on the surface in drying, another slide may be pressed on it gently so as to flatten it ; it may then be examined by the microscope, and any matter still obscuring it be removed. When it has thus been made sufficiently transparent, a little thick Canada balsam is to be smeared on the under surface, and by means of the ball of a finger moistened with spirits of turpentine, pressed on the slide so as to remove any vacuity of air-bubbles which otherwise make the preparation uneven under the microscope, and give the idea of a membrane or some different substance intervening amongst the rest. The next day a small drop of Canada balsam is to be smeared over the surface of the preparation with a finger, and immediately after two or three drops of spirits of turpentine ; and this process of applying the balsam and spirits of turpentine may require to be repeated two or three times for giving sufficient clearness, and guarding it from mould and atmospheric changes. Although it wants to be transparent, it does not require to shine as if it were varnished, and therefore the balsam and spirits of turpentine are directed to be wiped off with the finger. I have been several times very much mortified to find that the beautiful and delicate nervous roots had vanished after the application of Canada balsam, but have had afterwards the satisfaction of discovering that they returned after I had removed this with spirits of turpentine.

Another preparation may be made by cutting off a portion of the surface of the spinal cord, with very short ends of nerves attached to it, and very little of the medullary matter underneath. It is to be dried, and treated like the other preparations, and when finished it will show the nerves very beautifully, as they are collecting themselves into fasciculi ; and some parts of it will be found only a coarser representation of the more internal roots. Another preparation of a similar kind may be made by leaving the medullary surface uppermost, when nearly the whole of the medullary matter has been removed.

It is often necessary to put the spinal marrow in water for taking away the extravasated blood, and then it becomes very soft, and probably will be spoiled : in this case, or if putrefaction be approaching, it may be put into a mixture of two-thirds of common vinegar and one of alcohol, and may remain in this, after it has been cut into pieces, for a few days or weeks, and then be made into preparations, but it is much longer in drying than by the first method. This process is worthy of particular attention, as the difference between the white and grey matter is well preserved by it, and the preparation capable of being made thinner and more transparent ; consequently, however, more care is required in removing the soft matter underneath, and performing the other manipulations. The preparation thus made is also more quickly finished ; for when all the opaque matter has been removed

by the scalpel and spirits of turpentine, the Canada balsam may be at once smeared on under the surface, and the piece be placed permanently on the slide.

I have, according to the preceding directions, acquired about a hundred very interesting preparations; and although they have cost me almost infinite time and pains, I feel amply rewarded, by having succeeded in securing one of the most interesting points of anatomy far better than I had previously done by any other means. [*Medical Gazette*, October 12, 1849.]

STATE OF THE FIBRIN IN BLOOD.

M. HORN affirms that the fibrin in the blood is united to the corpuscles, and not free; his proofs are, that when frog's blood is filtered, the fibrin appears in the form of flocculi, or thready coagula, and the microscope shows that these are formed out of the granules which are met with in the blood, and have the characters of fibrin; moreover, when the fibrin is carefully removed by beating, these corpuscles can no longer be found. The fibrinous corpuscles are formed from the colourless molecules, found so abundantly in the lacteals and lymphatics. M. Horn believes that these molecules by aggregation produce true lymph-corpuscles, which, indeed, in their earliest stage, appear like conglomerations of extremely little molecules. The molecules dissolve away as the corpuscles progress, the corpuscles become flattened and smooth, and are, lastly, converted into blood-corpuscles. M. Horn considers, also, that pus-corpuscles are nothing but aggregations of these molecules; just as in the normal state the fibrin is converted into globulin, so in pathological conditions it is changed into pyin. (*Schmidt's Jahrbuch*. Band viii. 1848; as in *Provincial Medical and Surgical Journal*).

DR. C. H. JONES' RESEARCHES ON STRUCTURE AND DEVELOPMENT OF THE LIVER.

The researches of DR. C. H. JONES are contained in Part I. for 1849, of the *Philosophical Transactions*. We copy the following abstract and commentary from the *British and Foreign Medico-Chirurgical Review* for October.

"A survey of the various forms of hepatic structure has led the author to the following CONCLUSIONS:

"1. The liver, in all vertebrate animals, may be regarded as consisting of a secreting parenchyma and excretory ducts. These two portions of the liver are not continuous with each other, but disposed simply in a relation of juxtaposition; the substance of the lobules being made up of secreting parenchyma and of blood-vessels, whilst the ducts are confined to the interlobular spaces. 2. The action of the liver seems to consist in the transmission of the bile, as it is formed, from cell to cell, until it arrives in the neighbourhood of the excretory ducts, by which it is absorbed. This action is probably slow, and very liable to be interfered with, contrasting remarkably with the kidney, where a particular apparatus is added to ensure completeness and rapidity of action. 3. The secretion of the hepatic cells is very liable to be retained within the gland, either in the cells or in a free state. This circumstance, as well as its structural peculiarities, seem to point out the liver as approximating to the class of ductless glands. For the same reason, it seems highly probable that a part of the secretion of the cells is directly absorbed into the blood which traverses the lobules. 4. In a classification of the true glands, the liver seems to occupy the lowest position, the highest being assigned to the permanently tubular, such as the kidney and testis. 5. From the condition of the secreting parenchyma in many instances, we learn that the secretory process by no means requires

the formation of perfect cells in order to effect its peculiar changes ; these may certainly occur in blastemataous matter, if a nucleus only be present. 6. The condition of the liver is in great measure dependent on the intensity of the respiratory process ; its products being unused, accumulate in the gland, often to a remarkable extent ; its function is therefore not only vicarious of respiration, as formerly supposed, but preparatory, and to some extent subsidiary.

"In an appendix to the paper, Dr. C. H. Jones thus replies to an objection which may be urged against his views, on the ground of the very small extent of contact between the ultimate ducts and the cells of the liver, which makes it difficult to understand how the bile which they secrete should be received into the efferent ducts. 'In answer to this I observe:—First, that I believe much of the secretion of the cells is directly absorbed into the blood traversing the lobules. Secondly, that I think it is by no means clearly proved that the secretion of the cells is perfectly-formed bile ; in many instances it clearly is not ; thus in most fishes, and in the fatty liver of the human subject, it is evident that the gorged parenchyma is full, not of bilious but of oily matter, *out of* which, however, healthy bile is elaborated. I have carefully examined the cells of the human liver, in organs which were quite healthy, without finding any evidence of the presence of bile in their contents ; though, in congested livers, the yellow molecules are often very distinct in the interior of the cells. In the pig, rabbit, and dog, the cells appear as pale granular bodies, and do not, as far as I can perceive, exhibit the least biliary tinge, even after the addition of nitric acid. In the sheep, there is generally a good deal of oily matter in the cells, but no biliary. I do not of course deny that bile is often found in the cells, especially in states of congestion ; but I conceive that in the perfectly healthy state, the complete elaboration of bile is effected principally by the nuclei of the terminal portion of the ducts.

"[Some of these statements are rather startling, and we can scarcely reconcile them with our own observations, which have never led us to entertain any doubt that, in perfectly healthy livers, the cells of the hepatic parenchyma contain true biliary matter. Again, we must take leave to question the statement that biliary matter can be directly elaborated from oily matter ; since we think that there is neither chemical nor physiological evidence to prove that such transformation is possible. On the contrary, there is much evidence of their entire distinctness of character and function ; and if the blood do absorb any of the contents of the secreting cells, in its passage through the parenchyma, as suggested by Dr. C. H. Jones, we suspect that the matter thus taken up will be the oleaginous, rather than the true biliary. *Editor of Brit. & For. Medico-Chir. Review.*"]

TRANSPLANTATION OF TESTICLES.

In *Müller's Archives* is a most interesting paper with this title, by PROFESSOR BERTHOLD, of Gottingen. He castrated six young cocks, of two and three months old, leaving the wattles, combs, and spurs untouched. From two of them (*a* and *d*) he removed both testicles. Thenceforth they took on the nature of capons, fighting very seldom and feebly, and giving the well-known monotonous capon crow. Their combs and wattles were pale, and little developed, and the head remained small. About five months after, they were killed ; a small scar occupied the place of each testicle, and the seminal duct had degenerated to a mere thread. From two others (*b* and *e*) only one testicle was removed, the other left in the belly. In two others (*c* and *f*), both testicles were excised, but one belonging to *c* was transplanted into the belly of *f*, and, *vice versâ*, one of its testicles was transplanted into *c* ; thrust amongst the bowels, and left there. All four retained the characters of uncastrated fowls ; they crowed lustily ; frequently fought with each other, and

with other young cocks, and exhibited the ordinary inclination for the hens. Their combs and wattles developed like those of others.

The cock *b* was killed two months after ; its single testicle was in the ordinary place, but had hypertrophied, and, on section, a white fluid exuded, which contained cells, but no spermatozoa. On the same day, the remaining three had their well-developed combs and wattles excised. The cock *e* at the same time, was deprived of the remaining testicle ; and *c* and *f* examined in the ordinary situation in vain for the transplanted organ. The now fully castrated *e* never grew comb or wattles, it ceased to concern itself about the hens, and fought no more with its own sex. *C* and *f*, however, reproduced both comb and wattles, and preserved their ordinary chivalrous demeanour. They were killed six months after the transplantation. In *c* the testicle was found behind the colon and between the ends of the cæca. In *f* it was nearer their middle, but otherwise in the same situation. They were of large size, and received large branches of the mesenteric vessels, which passed towards them. entered, and then took the ordinary course, in relation to their seminal tubes. On incising them, a normal seminal fluid exuded with the ordinary cells and spermatozoa. The author concludes :—

1. That the testicles are transplantable, and reunite with living tissues after their separation from the body, not only at their ordinary site, but in an abnormal situation.

2. Like the grafted tree, the organ on this new place still preserves its specific properties, and secretes its specific fluid.

3. It is well known, that after a division, a re-union of nerves restores sensation and movement. And it follows, from these experiments, that as the re-union could not have been one of those originally divided, there are no specific seminal nerves ; that nerves only are requisite—a strong, nay, almost a fatal objection to the theory, that would constitute the sympathetic a trophic nerve, or a nerve in itself specifically organized with reference to nutrition. [*Medical Times*, July 21, 1849.]

IMPORT AND DEVELOPMENT OF THE UPPER GERMINAL LAYER IN THE OVUM OF VERTEBRATE ANIMALS.

DR. REMAK has made the following communication to the Berlin Academy. When the scutiform thickening of the germinal disc appears (the “embryonalschild” of Baer,) three definite layers may be distinguished in the germinal disc. In this thickening, only the upper and the middle layers partake : the under layer being not concerned in it. This latter layer, which forms the epithelium, not only of the intestinal tube, but also of the air-tube, the cellular parenchyma of the liver, the pancreas, the kidneys, the thyroid, and the thymus, I propose to name the *gland-layer*. The scutiform central parts of the upper and middle germinal layers grow together in their long axis. By this confluence, commences the axial plate or primitive streak of Baer, from which proceed the medullary plate, the primordial vertebral plates, and the chorda. The medullary plate is thus connected with the free part of the upper germinal layer, while the primordial vertebral plates are connected with the middle layer. Both upper and middle germinal layers show a thickening, which encircles the axial structures, and is a residue of the double shield, not partaking in the forming of the axial plate. (Wolf’s “lamina abdominalis.”) The author has not been able to convince himself, that a process of the free part of the upper layer covers the medullary plate.

That free portion of the upper germinal layer which limits the medullary plate is neither,—as Pander, Baer, and others interpreted it,—the foundation of the abdominal walls, (“serous or animal layer;”) nor yet, as others have asserted, a transitory enveloping membrane. But it covers the embryo, and is thus the *foundation of the non-vascular and nerveless tegumentary*

coverings, the epidermis, the nails, the feathers, and the beak. The peripheric part clothes the cavity of the amnion; and, after the conclusion of the amnion, a process which separates itself from this, and surrounds the yolk, forms the so-called serous membrane. The name "horn-layer" (*hornblatt*) might best serve for this part of the upper germinal layer, which takes no share in the axial structures.

During the third day of incubation, the costal plates separate from that thickened portion of the middle germinal layer which defines the primordial vertebral plates; they are then apposed to a correspondingly thickened part of the horn-layer, so as to give rise to the cavity of the belly. The horn-layer thus loses its individuality, and becomes a covering of the costal plates. The limbs which spring from these latter, in growing, carry before them this covering. Even at the seventh day, a considerable thickening may be seen at the free ends of the hinder extremities, corresponding to the foundation of the nails.

It is not difficult to follow, throughout, the metamorphosis of the horn-layer into feathers, nails, and epidermis.

The feathers first appear as wart-shaped excrescences of the skin, which forthwith take a tufty or hairy appearance. Every such excrescence consists of a soft bolster, which is formed of cells, contains loops of blood-vessels, and is covered by a solid and proportionably thick covering derived from the horn-layer. On treating this latter with water, cells may be recognized on its outer surface. When the excrescence becomes longer, the horny covering acquires a disproportionate thickness. At the tenth day, this covering offers a singular contrast between an inner, solid, and opaque layer, which consists of columns projecting from the vascular axial space, and an outer transparent layer of cells which is loosened by water. Almost always, numerous star-shaped pigmentary figures are discernible in the inner layer; it is only in the quite white feathers that they are entirely absent. This layer, lying next to the vascular stem, is the groundwork of the feather, while, on the other hand, the outer epithelial layer is the basis of the colourless skin, on the rupture of which the coloured feathers come to light.

The commencement of the nails is similar. On the eleventh day the horn-layer is thickened on the points of the toes; and, from the twelfth day forward, one may perceive the separation of this layer into a solid horny layer of nail, and a soft, easily separable covering. Here also a very extraordinary histogenetic contrast is exhibited between the cells of the nail, and those of its tegumentary covering. In the transparent cells of the latter, which are dilatable by the action of water, the nuclei may be seen as proportionally small, solid corpuscles. Towards the sixteenth day, the hardened nail-layer exhibits large, transparent, vesicular nuclei, which are surrounded by a dense, finely granular cell-content. In the horny coverings of the foot, in the beak, and in the epidermis generally, the same separation of the horn-layer into two strata may be discerned.

Since the sweat-glands and sebaceous follicles of the mammalian skin are absent from birds, and research on the glands of the rump has hitherto afforded little result, it will require a comparison with the mammalian embryo, to decide whether the horn-layer partakes in the development of the cutaneous glands, and is thus, in this additional respect, comparable with the gland-layer of the intestinal tube. [Here a foot-note by the author states, that, in the embryo of the pig, he has seen the sebaceous glands produced from the tubular hair-germs, which are in their turn developed from the deep, pigmentary part of the horn-layer.] But, in any case, the observations now communicated exhibit a novel and surprisingly simple law of development for the higher vertebrata—to wit, a middle layer which develops the nerves and vessels; a central nervous system; and two non-vascular and nerveless outer layers. (*Müller's Archiv.* Heft. II, 1849, as translated in *Medical Times*.)

PRACTICE OF MEDICINE AND PATHOLOGY.

DIABETES MELLITUS IN CHILDREN, TREATED SUCCESSFULLY BY
SULPHATE OF IRON.

DR. N. HEINE, of Berlin, has recorded, in the *Journal für Kinderkrankheiten*, for May and June 1849, two cases of Diabetes occurring in children. The following is a slightly abridged translation.

DIABETES is one of those obscure diseases, the nature of which, in spite of all our endeavours, and notwithstanding our improved knowledge of therapeutics, is as little known to us, as if it had just commenced to occur. We know that it is an excretion of sugar in the urine, not resulting from disease of the kidneys, but from some chemical action in the blood; but beyond this, we know nothing. The expression Diabetes itself shows our ignorance of the subject; for it may be applied correctly to *Diabetes albuminosis*, or to *Diabetes insipidus*, as well as to *Diabetes mellitus*. While albuminuria, and sometimes simple diuresis, are not rare in children between five and fourteen years of age, Diabetes mellitus has been seldom met with—indeed never in children under five years old. Prout (on *Stomach and Renal Diseases*, 5th edit., p. 30, note) says, that in seven hundred cases of Diabetes mellitus, which had come under his observation, he met with only twelve persons from eight to twenty years of age, and only one under eight—a child five years of age.

We have observed two cases of Diabetes mellitus in children; and we consider them worthy of being recorded, partly because they both show that a profound derangement of the functions of digestion and assimilation was, without doubt, one of the causes of, or at least in intimate relation with, the disease; and also because, in both, the promotion and improvement of digestion, by the use of sulphate of iron, had a remarkable effect.

CASE I. Albert Arnold, aged nine years, the son of a dealer in victuals, was strong and healthy when born. He was nursed by his mother, who appears to have been, at that time, somewhat troubled with a cough, and to have died of phthisis two years after his birth. Up to four years of age, he was quite healthy: he then had measles, and thenceforth began to grow sickly. He appears to have been neglected by his step-mother: he was sent to one of the lowest schools, and had to go in all weathers, scantily clothed, to and from the school, which was at a considerable distance. In the morning, he had a crust of black bread at school: at noon, he generally had potatoes or turnips, but no meat. He was also obliged to eat this food very quickly. Now and then his father gave him dainty bits, generally consisting of hard indigestible substances, as old fat sausages, bacon, etc. Sometimes, also, he got from his father a small glassful of brandy to refresh him. All this operated very injuriously on the boy: he became weakly, lean, melancholy, and dull; he was constantly tired and wearied; his appetite became irregular, sometimes entirely failing, sometimes being so voracious that he would eat four or five slices of bread without being satisfied. At the same time, the alvine evacuations were very scanty. On two occasions, he had attacks of spontaneous salivation for eight or ten days, during which the saliva continually streamed, and flowed from his mouth while he was asleep. Purgatives alone had the effect of arresting it. After some time, the boy became feverish in the evening, and was no longer able to go to school. He had very little medical treatment; but, as he appeared still to grow worse, and had to get out of bed about ten times every night to pass urine, thus disturbing the rest of his step-mother, she determined to take him to a physician. She came to me with him; and I ascertained that the boy was labouring under a diuresis, which a chemical examination of the urine showed, without a doubt, to be Diabetes mellitus. Sugar was detected in large proportion in the urine.

I commenced the treatment with small doses of alteratives : I prescribed very small doses of calomel, with rhubarb and magnesia, till the stools were somewhat increased : in the intervals, I gave doses of grs. x of phosphate of soda, with gr. ss. of ipecacuanha. The tongue gradually became cleaner : the boy appeared to have less thirst, and the appetite became more regular ; the alvine evacuations were more copious, and the quantity of urine excreted was less. He was not obliged to rise so often at night. I gradually left off the calomel, but continued the phosphate of soda and ipecacuanha for some time, and then commenced the use of sulphate of iron. The boy had taken this for scarcely two days, when a remarkably rapid improvement appeared. He passed urine much less frequently, and had to rise only twice, at most, in the night. On being tested, the urine presented no trace of sugar. The patient continued the use of the sulphate of iron for five or six weeks, desisting for short intervals, and completely recovered.

In the following case, the sulphate of iron appears to have had an almost specific effect :

CASE II. Daniel Abrahamson, aged seven years, was brought to me by his mother. This patient appeared much emaciated ; his eyes were sunken, and surrounded by a blue margin ; his flesh was flabby, and his tongue much loaded ; he had a disagreeable sweet taste in his mouth, and appeared dull and low-spirited. His mother, a Jewess, informed me that she had been, with her two children, in search of her husband, who, she had heard, was in good circumstances in London ; but she had been unsuccessful, and was in great poverty. While there, she and her children were seized with fever. She was in an hospital for four or five weeks, during which time she was frequently delirious. In the mean time, one of the children—a girl—died of fever produced by starvation, and the boy was dangerously ill in the same hospital with herself. She was assisted, by some charitable persons, to return to Hamburgh, and thence to Berlin, where she was provided with gratuitous medical attendance, medicines, and nursing. I soon ascertained that the patient was suffering from Diabetes mellitus : he passed four or five quarts of urine in twenty-four hours, in which a large quantity of sugar was detected. At the same time, the intestines were torpid ; digestion was impaired, the appetite failed, or was perverted, and the thirst was very intense. I prescribed pills of compound extract of rhubarb, with sulphate of iron. The latter was given at first in doses of half a grain, and gradually increased. Matters amended with remarkable rapidity under this treatment. By the end of five days, the quantity of urine was diminished ; and on the eighth day, no trace of sugar could be detected in it. The appetite improved, and the strength of the patient increased. When the appetite and digestion had improved, more nutritious food was gradually given, until he was able to take meat and wine. After a treatment of about four weeks, the boy was perfectly cured.

We do not know whether sulphate of iron would have such a beneficial effect on adults and old persons, as on children. In the latter, it is worthy of closer investigation ; and we have not been able to forbear from bringing this medicine prominently into notice.

ERYTHEMA CHOLERICA ("ROSEOLE CHOLERIQUE" OF RAYER).

M. LE GOULPIS directs attention to this affection in the *Revue Médico-Chirurgicale* for October, 1849. He considers that it is of importance in the prognosis, and that it is in the strictest sense of the term a critical occurrence. The paper possesses considerable interest. Dr. BARTH details two interesting cases of this description in his valuable memoir contained in the *Archives Générales de Médecine* for September, 1849, entitled, "*Le Choléra Morbus observé à la Salpêtrière pendant les mois de Mars et Avril, 1849*".

SURGERY.

MAXILLARY NECROSIS OF LUCIFER-MATCH MAKERS: PROPHYLAXIS BY INHALERS, AND BY TURPENTINE EXPOSED IN SAUCERS.

In the LONDON JOURNAL OF MEDICINE for October, page 961, we gave an account of the views which Dr. Helft entertains regarding this affection. We have now to lay before our readers abridgements of two papers on the Maxillary Necrosis of Lucifer-match makers, the first by MR. HENRY TAYLOR, of Nottingham, and the abstract of a lecture at St. Bartholomew's Hospital, by MR. STANLEY.

MR. HENRY TAYLOR'S PAPER. CASE I. Henry C——, aged forty-nine, a lucifer-match manufacturer for the last eight years. He found no material injury from the work till March 1848, when, after suffering some degree of pain, he observed a discharge of purulent matter from his mouth, arising from the front of the upper jaw. This pain was of an acute character, and he attributed it to having decayed teeth. I saw him for the first time on December 1, 1848; he then presented a portion of his upper jaw-bone, which had become necrosed, had separated, and been thrown off by the unaided efforts of Nature. It measured in length around the arch two inches and a half, and in depth one inch; it embraced, in one piece, nearly the entire of the bodies of the two superior maxillary bones, with the alveolar cavities for the four incisors, two canine and four bicuspid teeth; it formed the floor of the nostril, and anterior part of the roof of the mouth, and extended backward nearly to its junction with the palatine bones. The cavity left in the mouth by this removal of bone was considerable; but, by applying frequent pressure to keep the soft palate raised, and by washing the mouth with detergent and astringent lotions, it soon healed, and left much less deformity than was expected; no reproduction of bone to any evident extent took place. At the time of his first presenting himself to my notice, he was very enfeebled, highly nervous, thin, emaciated, sallow, and of a pasty countenance. He was put upon generous diet, with tonic and stimulating medicines. Strict injunctions were given to keep himself from the workshops, and to take moderate exercise in the open air daily. His health improved; but I soon observed that the lower jaw on the right side began to swell. The gums were red and inflamed, and had a spongy appearance; he complained of being constantly cold and shivering, but had no distinct rigors; there was great tenderness of the jaw, which he could scarcely bear to be touched. A portion of dead bone showed itself on the inner side, close by the second molar tooth; a loosened molar tooth was extracted, local and general antiphlogistic treatment was adopted, with the external application of fomentations, poultices, etc. He now for some short time, took medicines of an aperient and anodyne character; but his system soon became so exceedingly nervous and irritable, that he had to resume the tonic and stimulating treatment. He was fearful of having the most trifling examination of his jaw made, and he refused any surgical treatment. Symptoms of extreme general debility and prostration of strength soon after showed themselves, attended with profuse diarrhoea and harassing tenesmus. The symptoms noted on January 11, 1849, were—Great swelling of the lower jaw, extending up the side of the face; excruciating pain up the side of the head, including the ear; numbness of the lower lip, on right side, not extending past the mesial line; breath exceedingly offensive, with a profuse discharge from the mouth of saliva, mixed with foetid matter; cannot swallow more than thickened fluids; pulse feeble; bowels relaxed; urine scanty and high-coloured; suffers but little from thirst; is highly nervous; jumped out of bed in the night, in a state of delirium; hand tremulous; unable to hold a

spoon with any liquid. Beef-tea, jellies, etc., were given with wine, and beside the anodyne and strengthening medicines, injections with starch and opium were administered, from which great comfort and relief were derived. Some few days after, matter formed at the base of the jaw, which was evacuated. The basis of the bone was distinctly felt necrosed, but he made a positive refusal to more being done than the insertion of a probe. His health afterwards very gradually improved, although his system continued weak and unnerved. The violent diarrhœa, tenesmus, etc., subsided, and he was enabled to take his food as usual; but the wounds continued open, discharging matter occasionally mixed with blood, which at times was very profuse.

On September 25, 1849, he again came to consult me, having had, for the two or three preceding days, much pain on the left side of the lower jaw, which was considerably swollen. The substance of the bone at its base felt materially thickened, and there was every appearance as if similar mischief had commenced in the body of the bone on this side, to that on the opposite. His pulse was quick and feeble, and his whole system indicated great constitutional disturbance; indeed, there was every reason to believe that the poison had powerfully manifested its destructive effects on this side also, although for the last ten months he had entirely withheld from work, and but seldom entered the workshops. The portion of dead bone exposed to view within the mouth, on the right side, had much increased, being at least one inch in length. The discharge from the wounds had rather decreased in quantity.

CASE II. James B—, aged forty-five, superintending assistant to Mr. C—, who has the credit of introducing the system of lucifer-match making into England. Has been engaged in this work continuously for fifteen or sixteen years; he is of a sanguineous temperament; of a full, plethoric, and gouty diathesis, and habituated to excess in drinking. He was seized with pain and swelling of the lower jaw, on the left side, in the early part of the month of July 1849. When he first came to consult me, the swelling extended far up the side of the head. I found the bone highly inflamed and thickened, being excessively tender to the touch. Upon making a careful examination of his mouth, I found the gums looking red and spongy, and separating from the teeth, having an appearance somewhat similar to when mercury has been taken in a sufficient quantity to produce ptyalism; but the marginal ulceration was not so distinctly marked; the teeth on the side affected, loosened, including two of the incisors, the whole of which were in a fearfully neglected and encrusted state, and the breath was sadly offensive. Most of the loose teeth were removed, several being easily extracted with the finger and thumb. Antiphlogistic treatment, with purgatives and salines, were prescribed, with the external application of fomentations and poultices. He continued under my care till the latter part of the month, when he left this neighbourhood, and went to his friends in London, where he soon after placed himself under the care of Mr. Simon and Mr. Dixon, in St. Thomas's Hospital, from whom I have learnt that the bone has since become extensively diseased.

Remarks. Cases of a similar kind to those related above have fallen under the observation of, and been published to the medical world by, Drs. Heyfelder and Dietz, of Nuremberg, and F. W. Lorindser, chief surgeon in Vienna—places where the phosphorus match manufacture is carried on extensively. In this country, Professor Taylor, in his admirable work on poisons, has briefly referred to the subject; as also Mr. Stanley, in his recent able volume on Diseases of Bones.

The work in which the two preceding individuals were engaged being of a comparatively new character, it may be desirable to show to what extent their bodies were exposed to the poisonous vapours of phosphorus, whilst occupied in their several vocations.

The duties which each of them undertook to perform were those of "the dipper." Phosphorus, combined with oxymuriate of potash, glue, etc., made into a paste, is placed on a metal plate, at a temperature sufficiently high to keep it liquid. Into this preparation on the heated plate is dipped the bundle of matches, ready prepared with sulphur; so that the dipper has to stand over the plate, and inhale the abundant fumes which are eliminated. So completely are the clothes and dress of the person impregnated with the phosphorus, that at night, in the bed-room, when dark, they appear incandescent. Beside the quantity which has to be inhaled during the various dippings (which, in Mr. C——'s factory, continues for half-an-hour, at three or four different times of the day), the person has to prepare the compound for the dipping, during which process much of the phosphorous acid is given off. During the drying of the matches when made and dipped, the vapour is eliminated in considerable quantities into the same room in which the usual work is carried on. Lately, however, means have been contrived, in this factory, to prevent so great an inhalation of the poisonous fumes; and I have further suggested, what in all probability will be carried into effect, that a mask be worn over the face of the dipper, which shall have at the end a tube to pass out of the building into the open air, which mask is to be provided with valves for exhaling and inhaling, like those attached to the chloroform inhalers.

It is evident, from the two preceding cases, that phosphorus, when imbibed into the system in a slow manner, acts as an irritant poison.

There was doubtless, in each case, great constitutional disturbance, and the effect upon the nervous system was very marked. At times, C—— suffered severely from nervous twitchings of a very distressing character, extending down his legs, awakening him out of his sleep. He complained of overpowering depression of spirits, being, as he expressed himself, "quite unmanned".

No excess of mercury or arsenic had ever been taken into the system in either of the above cases. M. Dupasquier believes that this disease of the bones is not dependent upon the vapour of phosphorus (which he considers as possessing no poisonous action), but upon the presence of arsenic, which he believes to be used in the manufacture of lucifer matches. In this opinion, I can scarcely anticipate he will have many followers; for in each case I know of, there was an absence of cardialgia, vomiting, inflammation of conjunctivæ, suffusion of the eyes, etc., symptoms which would have been present had arsenic been the poison which had slowly been received into the system.

It is most assuredly a singular circumstance, that phosphorus, which holds so important a part in the structure of bone, in form of phosphate of lime, should produce such a destructive action upon bone, when received into the system in excess. Professor Taylor says, "these effects have been attributed to the respiration of the vapours of phosphorus, which are supposed, by becoming acidified, to act chemically upon the bones".

Further interest is attached, in the consideration of these cases, to know how far the combination of sulphur with the phosphorus may increase the destructive influence of the vapour. Leopold Gmelin, the Professor of Chemistry in the University of Heidelberg, in his *Hand-book*, lately translated by the Cavendish Society, points out in a very extended manner, the numerous combinations of sulphur with phosphorus, and shows the various compositions and decompositions which occur when they are mutually acted upon. This point I must entirely leave to the more profound knowledge of our chemists; there is, however, abundance of facts demonstrated, showing that some of the various gaseous products are extremely volatile and destructive.

An attentive chemical enquiry may be the means of throwing some light on the rationale of the *treatment* which ought to be pursued in the early stages. My friend, Dr. Hutchinson, whose opinion I at all times most highly appreciate, in a note to me, says,—"The more I think of the case, the more

I feel inclined to believe in the importance of administering freely the muriatic acid ; if in any way we are to be influenced by the effects it has upon dead bone, there is a little reason to suppose it may have some slight effect upon what appears to be the poisonous influence upon bone, of an excess of phosphorus, and either circulating in the system, or locally absorbed by those bones most exposed to its influence ; it may also be of service by exciting a general beneficial constitutional effect”.

MR. STANLEY'S LECTURE. MR. STANLEY brought before the pupils of St. Bartholomew's Hospital, on November 10, a patient who had lost the whole of his lower jaw by Necrosis. The man, who looked about five or six-and-twenty, had been six months in the hospital, and traced the affection of the jaw to the vapours of phosphorus, to which his trade of a lucifer-match maker had exposed him. The whole body of the lower maxilla, with one of the condyles, were exhibited, and presented several necrosed fragments, in which the original shape of the bone could easily be recognised. Mr. Stanley mentioned that the condyle, which was wanting to complete the necrosed jaw, had very probably been absorbed. The patient's appearance did not, at first sight, betoken such considerable osseous loss, and the usual contour of the face was tolerably preserved. This small amount of deformity was accounted for, by Mr. Stanley, by the thickening of the tissues in contact with the jaw, and an abundant fibrinous deposit which Nature had thrown out, in her endeavour to repair the loss of bone. New bone, however, is never thrown out in this form of Necrosis. Mastication, in this instance, is carried on tolerably well ; the food must be reduced into small fragments, which the patient prepares further by pressing them strongly against the roof of the mouth with his tongue. The function of nutrition seems to be going on tolerably well with this patient, for he looks in pretty good health. It may, then, be inferred, that this imperfect preparation of the alimentary bolus proves sufficient for the digestive functions.

The painful feelings excited by the sight of this new victim of our vaunted improvements in various branches of manufacture, were greatly mitigated by the announcement Mr. Stanley made regarding the prophylaxis. Workmen in lucifer-match manufactories have now a chance of escaping the baneful effects of the evolution of phosphorous acid, by placing saucers filled with oil of turpentine about their work-rooms. As oil of turpentine is a solvent of phosphorus, it is expected that it will absorb the vapours which do so much mischief. This precaution is taken at a large lucifer-match manufactory in the neighbourhood of the London Hospital, and the very best results are expected from it. This case, so instructive in itself, was rendered peculiarly valuable in being brought forward by the distinguished author on *Diseases of the Bones* ; and we were sorry that the time and place did not allow Mr. Stanley to enter fully into the different questions which such a case naturally gives rise to ; for instance, as to whether the fumes act primarily on the periosteum, or whether the jaw is secondarily affected. This question (left unsettled in the work just alluded to) appears of some importance ; for if the constitution be affected first, the poison might be counteracted by throwing appropriate chemical agents into the blood. The two cases mentioned in the *Lancet*, (Nov. 10, p. 498,) by Mr. Henry Taylor, of Nottingham, will prove very valuable to those who may investigate this subject in all its bearings. Two men, who were employed for a considerable time at the manufactory before they experienced any ill effects,—one in the upper, and the other in the lower jaw,—suffered Necrosis. These men were “dippers” ; and it is but fair to suppose that the hand would have been the first to suffer. had the effect been direct. It must, however, be confessed, that the fact of the upper maxilla being affected, in Case 1, militates against the following passage, which we find at p. 75 of Mr. Stanley's treatise : “ Against the opinion that the phosphoric vapour acts merely as a local excitant, the objection

has been urged, that it produces no effect on the periosteum of the bones of the nasal passages, through which the vapour is directly inhaled." That these cases should not be classed among ordinary Necrosis of bones, is sufficiently obvious, the entire absence of the least attempt at the regeneration of bone being the most characteristic differential feature. Nor could this regeneration well take place, as, to use Mr. Stanley's words, (p. 75,) "there is here a total want of the essential conditions for the reproduction of bone—namely, inflammation in healthy structures, with health in the general system." The grey, pumice-stone-like, newly-formed osseous substance, found by Dr. Heyfelder on the outer surface of the portions of bone which he extracted in similar cases, likewise points, as Mr. Stanley remarks, to an affection distinct from the usual death of bone. [Abridged from *Lancet*, Nov. 16, 1849.]

CLEFT PALATE : AND THE OPERATION OF STAPHYLOGRAPHY.

THE LONDON JOURNAL OF MEDICINE for January and February contained Mr. FERGUSSON's admirable memoirs on this subject. In the second paper, p. 123, he gives a note from MR. R. QUAIN, intimating his approval of the method of preliminary section of muscles. Mr. Quain has, in the *Medical Times*, recently given his opinions at greater length, in connexion with the history of two cases of Cleft Palate requiring different modes of treatment. An abridgment of this interesting paper is subjoined.

CASE I. William Turner, aged 16, admitted into hospital to be treated for Cleft Palate. He is of ruddy complexion, somewhat strumous in appearance. Has usually had good health, but the cervical lymphatic glands generally are enlarged; and there are now discernible at the side of the neck below the parotid, the scars remaining from suppurating sores, which formed in connexion with inflamed glands two years ago. In infancy, when he was suckled, the milk flowed from his nostrils "almost as fast" as he drew it into his mouth. But the quantity that thus escaped gradually lessened as he advanced in age, so that by the time he could feed himself, very little passed by the nose. At present none of his food is diverted from its proper course. His speech is mostly unintelligible, except to those who have been accustomed to it. The soft palate is divided in its whole length into two equal parts. The parts are red-coloured, and of good thickness. At the upper or anterior end they are attached close together to the hard palate. From this they diverge at once, and hang wide asunder; but, when upon being bid, the young man makes an effort to swallow, they are observed to approach one another, and even to touch at the lower ends—not in their whole length. The bony palate is entire, except that there is a very small angular notch at the middle of its posterior margin, where the flaps of the velum are close together. The tonsil on the right side is enlarged.

By means of a scalpel, curved on the flat, and double-edged, a free incision was made upon the back part of each of the two halves of the soft palate, at about half an inch from the edge. Some difficulty was experienced in effecting this first step of the operation on the right side, in consequence of the flap of the velum being forcibly drawn outwards to the side of the pharynx, and much narrowed. This happened especially when the part was touched with the knife; and, at the same time, a narrow depression or dimple was observed on the under surface of the palate, doubtless marking the insertion of the levator palati on the posterior surface, the muscle being in a state of forced contraction.

In the next place, three silk sutures of good thickness were introduced, the lowest through the uvula near its base, the highest about half an inch from the hard palate. They were passed by means of a bent needle, of the kind constructed with a long handle and of rather small size. The needle carrying each suture was made to puncture the left half of the palate on the lower or buccal surface, about a quarter of an inch from its edge. When the velum

had been penetrated, the loop of string was caught with a pair of forceps passed back in the cleft, and was so held while the needle was withdrawn. A loop of thin silk was passed in the same way, at the same height, in the opposite division of the velum, and this loop was used to convey the suture thread from behind forwards into the second half of that structure. The edges of the soft palate being pared, the ends of each of the sutures were drawn together and fastened with a surgeon's knot, a pair of forceps being used to prevent the ends of the threads from slipping while the knot was made.

On the evening of the operation, a degree of nausea came on, which was troublesome till the patient fell asleep. It was probably caused by a little oozing from the incisions on the back of the palate and from its edges. During the first following days, there was a good deal of swelling about the sutures. On the third day the highest of these was removed, and on the day after the other two were cut and withdrawn. It was now found that union had taken place to a considerable extent. The sides of the uvula, with those of the velum contiguous to it, were firmly united; and, about midway between this point and the hard palate, union was established for the space of about a quarter of an inch. Above and below the last mentioned adhesion there was an opening, and at the lower one of these a slight degree of ulceration was noticed. By the use of solid nitrate of silver, slightly applied when the surface became clean and repeated at intervals, the two openings soon became reduced to the size of pin holes, and these were closed after a heated wire had been passed through.

After the operation, and during several days, the patient was fed, at intervals of about eight hours, with bread in warm milk or beef tea. No solid food was permitted. Upon trial being made respecting the effect of the operation on the power of articulation, when the holes in the velum were closed, some improvement was evident, and it gradually increased up to the time the patient's attendance at the hospital was discontinued.

With a view to surmount the obstacles to the successful result of Staphyloraphy, as now detailed, modifications have from time to time been made in the operation; or rather, additions have been made since the operation was first introduced into surgery. Thus, in order to prevent or diminish the tension or dragging which was occasioned by the ligatures, incisions have been made into the velum outside the ligature holes. And in order to paralyze the muscular action during the healing process, the levator palati, palato-pharyngeus, and palato-glossus muscles have been divided. To Mr. Fergusson we are indebted for showing the necessity of dividing the muscles, and for suggesting the method whereby to attain the object.

It was then with a view to overcome the obstacles here noticed, that, in the case under our observation, an incision was made on the posterior or upper surface of each side of the velum palati, where the levator joins that structure; and, further, the incision was made of considerable length and depth. By an incision of this kind, not only is the muscle divided, but the over tension of the velum when the ligatures are drawn together is guarded against, as far, at all events, as any separate or independent incision for the purpose would attain this end. The single incision then accomplishes both objects.

No muscle besides the levator palati was divided. The reason is this: I am not satisfied of the necessity or utility of cutting the palato-pharyngeus and palato-glossus. They, in the healthy state of the parts, undoubtedly approach each other from opposite sides during deglutition, so as to close upon the alimentary bolus, and they are, I apprehend, the agents in that approximation of the two halves of the palate which takes place during the attempt to swallow the saliva. We witnessed this fact in Turner; it is common in such cases. Influenced by these considerations, I restrict myself to the division of the levator muscle. In making the incision for this

purpose, it will be well to draw the fragment of the velum, by means of the hooked forceps, into the pharynx; for by this means, the part to be divided will be stretched, at the same time that the tendency of the muscle to narrow the palate is counteracted.

There is no likelihood, I may add, of inconvenient hæmorrhage during the operation. The internal carotid, the only artery of importance in the neighbourhood, is considerably behind the seat of incision.

Much depends on the *proper application of the sutures*, and much attention is requisite in order that they shall be firmly fixed, retaining the flaps in apposition, but, if possible, without any straining of the tissues.

From the operation, even when completely successful, all that can be expected is, to place the patient in such a condition, that he may effectively practise to attain the art of speaking with distinctness. Turner, it should be stated, had less than average facility in speaking, even as compared with others affected with the same malformation. He failed especially in pronouncing letters which are formed with the tip of the tongue, and with the aid of the teeth. "Sister," "Thames," etc., were scarcely to be recognized. It seemed, in watching him, as if the muscles of the fauces were engaged in preventing the voice from passing through the nose, while those at the anterior part of the mouth were not called into action at all, or very ineffectually. Notwithstanding, however, the manifest want of aptitude in the patient, the amendment, which is stated, in the narrative read to you, to have been apparent when the sides of the palate had been completely closed, became a clear improvement after he had been engaged with his family in practising the pronunciation of some words, of which Mr. Jackson, his dresser, kindly gave him a list. The improvement, as might be expected, is more decided and rapid when the person is placed under the instruction of one of those gentlemen (Mr. Poett, for example) who devote themselves to correcting faults of pronunciation. But from this advantage, hospital patients are excluded by the expense.

CASE II. J. W., a female, aged 23, admitted into hospital on account of a congenital deficiency in the palate. When an infant, during the act of sucking, the milk used to pass into her nose. She could not speak at all until she was two years old, and then very indistinctly. In her childhood she was able to whistle and sing a little, and to blow out a candle. She could not, and cannot now, pronounce the letters *g, h, j, k, q, t*; but still she speaks with tolerable clearness. She has suffered some years from irregular menstruation.

The soft palate is entire at its middle, but the uvula is fissured, the two parts hanging close together, and at the upper end there is an oval opening, measuring more than half an inch in length. At each side is an elongated elevation upon the surface of the velum; this, at first sight, seemed to me to result from increased thickness, owing probably to the presence of greater amount of muscular substance; but on examining it with the finger, I came to the conclusion, that the appearance arose from a degree of looseness in the part, caused by the position into which it fell when under examination. Except at the lower part of the uvula, the whole structure was much thinner and paler than usual, and this condition was especially apparent at the edges of the oval opening, which seemed to be formed of nothing more than the double mucous membrane. The back part of the hard palate is defective, being widely notched up to within an inch and a half of the incisor teeth. The patient states, that the opening in the velum was originally round, and about the size of a pea; and that it had no more than this size till about two years ago, when an operation was performed by a surgeon with a view to close it. According to her own account, she has since that time experienced more difficulty in pronouncing words. She adds, that while at all times she was obliged to be careful in eating, she has, within the period referred to, been compelled to be much more so; and now, unless she holds her head up when in the act of drinking, the fluid passes into her nostrils.

A single suture was passed through the uvula after the edges had been pared. The parts became united in a couple of days without any difficulty. At a subsequent period, a stopper of India-rubber was inserted into the aperture, and the patient then left the hospital. Two months afterwards, the following entry was made in the Case-book :—"She attended for inspection this morning. She cannot keep the stopper in its place for more than four days at a time, her mouth then becoming sore ; she has no difficulty in removing and replacing it. Her pronunciation is evidently more distinct. She can pronounce all the letters that she previously could not, with the exception of the letter *t*."

I advised the use of a simple form of obturator for the opening which still remained. But, had the soft palate been of the usual thickness, and had there been also no material loss of substance around the opening, sutures would have been applied with every probability of a favourable result ; and, considering that in this case, a large part of the velum was perfect, it would not, in my opinion, have been necessary to attempt the division of any muscle. An incision to a considerable depth, outside the ligature holes, would, however, be useful on the principle explained at a former part of this lecture, viz., to diminish traction, and the consequent tension of the parts.

PREVENTION OF THE ENTRANCE OF AIR WHEN REMOVING FLUID FROM THE
PLEURÆ, PERITONEUM, AND CAVITIES OF ABSCESSSES.

At a meeting of the *Société de Chirurgie* of Paris, held on the 14th of November, a letter was read from M. RACIBORSKI, of which the *Union Médicale* for November 17th gives the following extract.

A wet, collapsed hog's bladder is fixed to the outlet of the canula which is to be introduced. When the trocar has sufficiently entered the cavity, the bladder must be supported by the left hand of the operator, the right being used to withdraw the trocar, and so allow the fluid to flow through the canula into the bladder. If the bladder be insufficient to contain the whole of the fluid to be withdrawn, the flow has to be stopped by pressing the side of the bladder against the outlet of the canula, whilst an assistant punctures the bladder in a convenient part, and thus evacuates its contents. By securing the opening by a ligature, the bladder may be made to serve for the evacuation of the whole of the fluid.

OBSTETRICS.

DR. E. J. TILT'S PAPERS ON ACUTE OVARIAN DISEASES.

THE importance we attach to the cultivation of the obstetrical branch of medicine is evident from the space we have allotted to it in our Journal. We have lately reviewed Dr. Tyler Smith's work, wherein the component parts of the science of midwifery were blended into one harmonious whole; we are now engaged in giving our readers an account of Dr. Bennet's *Treatise on Inflammation of the Womb*; and we have thought it not unsuitable also to give here a digest of DR. TILT'S important papers (*Lancet*, 1st vol. of 1849,) on the Acute Forms of Ovarian Disease. In doing this, it is needless to remark how cursorily this subject has been alluded to by writers, and how little it is known to the profession. We must, however, remember how small was our knowledge of the physiology of these organs until within the last few years; and that we are indebted to the labours of our contemporaries—Baer, Bischoff, Pouchet, Raciborski, Martin Barry, Robert Lee, etc., for having so completely changed the face of ovarian physiology. That some one should step forward and attempt to give a corresponding extension to our knowledge of the pathology of the same organs cannot, therefore, excite our surprise; and we must admit that Dr. Tilt (by his lucid arrangement of facts little known,) has done more than any English author to throw light on this obscure subject. His papers deserve to be placed along with the admirable monograph of M. Chereau.¹

Starting from physiological considerations, Dr. Tilt begins by pointing out the importance of the ovaries; which are, throughout the serial chain of beings, the primary organs of generation, to which are superadded, in the highest beings, a nidamental portion of the oviduct to perform the office of fruit-holder. He shows the monthly ebb and flow of blood towards those organs, the monthly elaboration which ends in the ulceration of a portion of their surface, and the numerous vicissitudes to which they are amenable from conception, pregnancy, and delivery; and then asks, if an organ endowed with such an amount of formative energy, so abundantly supplied with blood, and living so active a physiological life, is not to have its career chequered by a corresponding amount of morbid action? Answering in the affirmative, Dr. Tilt proves the assertion by referring to older writers, and to Morgagni, who says, "If I were to enumerate all the lesions of the ovaries and oviducts which I have seen in my dissections, this (my forty-third) letter would be the longest of all". He refers to Drs. Ashwell, R. Lee, and Copland, and to the Treatises on Pathological Anatomy and on Diseases of Women; but he cites more particularly those, who, like Ritchie, Barry, Negrier, etc., being engaged in minute physiological researches, have often detected minute testimonials of inflammation in small portions of the ovary, and principally in the ovarian vesicles; false membranes or pus, which, be the quantity found ever so small, sufficiently prove that they were secreted by an inflamed surface. Having, satisfactorily, proved the much greater frequency of Ovarian disease than is generally supposed, Dr. Tilt explains why certain of its forms have been so little recognized.

I. On account of the diminutive size of the ovaries, and their being so deeply seated in the abdomen, and so difficult of access.

II. Because the symptoms by which their morbid states are manifested, such as the pain in the back and the sense of bearing down—symptoms which are the expression of ovarian sufferance, reacting on the spinal marrow—have been so long called *uterine*, and therefore supposed always to denote uterine

¹ The work of M. Chereau (*Maladies des Oaires*. Paris: 1844) has not been translated into English, but an excellent analysis of it is given in the *Edinburgh Monthly Journal* for January 1845, p. 38.

disease, that the pathologist expects always to find in the uterus the cause of the disease,—a view of the case in which he is further confirmed when he detects proofs of uterine morbid action, which has been itself often directly caused by morbid ovarian irritation.

According to Dr. Tilt :—Subacute ovaritis, and inflammation of the neck of the womb, having some very characteristic symptoms in common, are often confounded with each other; and thus ovaritis is often really cured by the treatment made use of to remove inflammation of the womb. These views are well supported by physiology and pathology; and we have been forcibly reminded of their truth, by the perusal of revelations which Dr. Oldham has lately given to the medical public, in a valuable contribution to *Guy's Hospital Reports* for October 1849. A sterile lady came from Jamaica to London. She was quite well; but she had been told by her medical attendant in Jamaica, that if she placed herself in the hands of some of the eminent London practitioners, her marriage might become fruitful. She did so; and a London obstetric physician, believing with the Jamaica practitioner, that the opening of the womb was not sufficiently large, slit it up with a cutting instrument. The lady was then condemned to wear, amidst atrocious sufferings, the uterine stem-pessaries. Acute peritonitis was brought on by this treatment, and the patient died. Dr. Golding Bird, who had been incidentally called in, gave the history of the case to Dr. Oldham, and requested him to open the body. Death had been caused by acute peritonitis; the uterus was found sound; the Fallopian tubes were diseased; and the ovaries presented traces of sub-acute inflammation, sufficient to explain the long-standing sterility. This, we fear, is an authentic representation of many similar cases, which now remain unpublished by those to whom they occur: it shows the danger of such energetic treatment for an infirmity which is, after all, problematic in its nature.

Dr. Tilt divides Ovaritis into, 1, the sub-acute; 2, the acute.

Sub-acute ovaritis is defined to be, “inconsiderable swelling of the ovaria, with increase of heat and pain on pressure, accompanied by intermittent or permanent pain, or by uneasiness in the ovarian region, radiating to the loins and thighs, and producing, according to the constitution of the patient, amenorrhœa, dysmenorrhœa, sterility, or hysteria”.

By *sub-acute* inflammation, as distinguished from *acute*, Dr. Tilt does not imply any difference in the intrinsic nature of the morbid phenomena, but the delimitation of the inflammatory action to certain distinct parts of the ovaries, as the ovarian follicle, and to portions of the ovarian tissue so small, that they give rise to little swelling, and to no febrile action; and he points out, as peculiar properties of the sexual system in women, the liability to inflammation of certain portions of the generative intestine, in which the others may not participate—a peculiarity to which the ovary is still more liable, on account of its complex structure. Having already alluded to the pathological anatomy of sub-acute ovaritis, we pass over that section, and arrive at the causes, which are studied according to their nature and effects, with a care and minuteness not to be found in other works.

The causes are divided into the predisposing and the exciting. 1. The predisposing are, the function of the organs and the lymphatic temperament. 2. The exciting causes are, the arrest of the menstrual flow by the numerous means we are acquainted with: vulvar irritation, leucorrhœa, and inflammation of the neck of the womb, and also sometimes the very cauterizations which are employed to cure these diseases. Dr. Tilt quotes Gendrin as having seen cases of this description.

We must refer our readers to the original papers, for an account of the symptoms of the disease, and of the modes of exploration of these organs, in order to establish our diagnosis; and we must turn to what our author calls the “terminations”, but what we should rather term the different forms of ovaritis. Dr. Tilt carefully remarks, that he does not pretend that amenorrhœa,

dysmenorrhœa, sterility, and hysteria are always caused by sub-acute ovaritis; while he shews that the groups of symptoms called by these names can often be deduced from such a morbid condition; and that inflammation is the key-stone of ovarian pathology, as it has been proved to be the key-stone of the pathology of the uterus, and of all the other organs of the body.

With respect to sterility as produced by sub-acute ovaritis, as the author published a paper on the subject in the June number of this Journal, we now merely mention, that it produces sterility, either by causing the shedding of the immature ovula, or by thickening the ovarian envelopes, so as to preclude the exit of the ovula, which remain blighted in each follicular crypt.

With regard to hysteria, he disagrees with those authors, ancient and modern, who, with Hippocrates, place the cause in the womb; and he maintains that hysteria is intimately connected with certain inflammatory lesions of the primary organs of the generative system; but he does not pretend to explain how organs so small, and under the more particular influence of the ganglionic nervous system, can produce such extensive functional derangement of the cerebro-spinal system, and such sudden explosions of its disordered activity. With respect to the "treatment", our readers will find it detailed in the number of our Journal to which we have already alluded, or in the *Lancet* (April 21st, 1849), wherein it is given at greater length. The practical bearing of the conclusion of that paper is, we think, worth transcribing.

"We are thus led to consider two important questions—

"1. Is marriage to be sanctioned when the ovaries are subacutely inflamed?

"2. Is it to be countenanced when the ovaries are prone to be subacutely inflamed?

"In answer to the first question, we say decidedly, No! The disease, in the generality of cases, may be removed by proper treatment; and if it cannot be so removed, as in those cases occurring in delicate scrofulous girls, who from their infancy have suffered from peritonitis, or enlargement of the mesenteric glands, how cruel it would be to allow a marriage which must be attended by fatal consequences; for undoubtedly marriage would aggravate the disease, conception would be followed by abortion, and, should a sickly child be brought to light, its birth would generally be followed by the increased illness of the mother, and this would terminate in her sinking into a speedy dissolution.

"But to the second question we may answer in the affirmative, perfectly persuaded that it is the want of the appropriate stimulus to the ovaries which should promote their healthy action, that is often the cause of their becoming the seat of morbid affections. Should this, however, not be the cause of the ovaries being prone to disease,—and they are still liable to relapses of subacute inflammation,—it can then be checked by appropriate treatment. We believe that Nature, true to all her healthful impulses, promises the continuance of a greater amount of health to those who take upon themselves the burden of child-bearing and the perils of delivery, and that marriage is, in many cases, a preservative against hysteria and those spurious ovarian and uterine growths, before which the medical attendant will afterwards stand in powerless dismay. We here conclude our observations on the treatment of subacute ovaritis and its terminations; and before considering the acute form, we will pause a moment to impress again upon the mind of the reader the necessity of paying more minute attention to the detection of the obscure symptoms of ovarian inflammation, whenever called upon to prescribe for dysmenorrhœa, sterility, or hysteria, so that the evil may be attacked in the bud, and its increment forbidden, and not allowed to attain to such a degree of magnitude as may compromise the patient's life, either by its presence or its eradication. 'Principiis obsta; sero medicina paratur.'"

We have been detained longer than we intended on the papers relating to subacute ovaritis, because the facts and views therein expounded are not to

be met with in the best works on the subject, not even in Dr. Henry Bennet's work, *On Inflammation of the Womb and its Appendages*.

Dr. Tilt defines acute ovaritis,—considerable swelling of the ovaria and of the surrounding cellular tissue, with febrile reaction, formation of pus, its elimination and absorption.

There is nothing new under the head of pathological anatomy; and with respect to the causes, the author refers the reader to the causes of the sub-acute form, as the same agents give rise to both diseases; the increased energy of these agents, and a constitutional proneness to be affected by such causes, explaining sufficiently the production of acute ovaritis. The symptoms vary according to the size of the tumour, but we cannot enter into their details.

The terminations of acute ovaritis are stated to be—I. Resolution. II. Elimination. III. Metritis and inflammation of the oviducts. *Resolution* is admitted not to be uncommon, and even sometimes to take place when the quantity of pus is very considerable. *Elimination*. The pus has been known to find its exit by the skin, intestine, bladder or vagina, and into the peritonæum. The vaginal opening is the most felicitous, and the effusion into the peritonæum the most fatal. The author gives an interesting case, to prove the possibility of the transmission of inflammation from the inflamed ovary to the womb, and a sketch of the inflammation of the Fallopian tubes, wherein he shows how retention and effusion of the menstrual flow can produce certain pelvic tumours very little understood. When discussing the treatment, the author shews how the mercurial frictions and the rectal and vaginal injections advised in the sub-acute form are here inapplicable, on account of the inconvenience and pain they occasion. He recommends an active treatment; and with regard to the pelvic cysts, he advises to study where nature attempts to effect an opening, and to follow her indications, preferring to open such tumours by means of the repeated application of Vienna paste, to opening them by the knife.

Dr. Tilt deserves well of the profession for his elucidation of ovarian disease. The field he has chosen is too much neglected, and a complete treatise on ovariology is very much wanted. We hope that Dr. Tilt may continue his researches, and produce such a work as we allude to.

CAUSES AND TREATMENT OF PUERPERAL METRO-PERITONITIS: WITH THE STATISTICS OF THAT DISEASE IN THE HOSPITALS OF PARIS.

By DR. MAURAT.

THE paper from which we make the following extract is a thesis of considerable merit, which has been re-printed in the *Revue Médico-Chirurgicale* for September, 1849, pp. 129. The author's object is to explain and enforce the system of prophylaxis and treatment pursued at the Hôpital St. Louis, and to contrast the statistics of Puerperal Fever in that institution with those of the other Parisian hospitals. The discrepancy is striking; but much of the value of the lessons to be learnt from it, is lost by no detail being furnished of the treatment pursued in each of the establishments. After speaking of the predisposing, general, and occasional causes of Metro-Peritonitis, the author unfolds his views. The following is a translation of part of M. MAURAT's thesis.

The cause which, in the great majority of cases, gives rise to Peritonitis in Puerperal women, and which may be called the most essential cause, is a chill; with which, however, I would not have confounded the rigour which ushers in every local inflammation. So far back as 1573, Ambrose Paré made the first step to this discovery. "The recently delivered woman", said he, "must have the womb carefully preserved from contact with cold air; for, being empty and gaping after childbirth, it becomes readily filled with cold air, which induces swelling and distension, closes up the uterine sinuses, checks the lochia, and induces in this way engorgements

of the womb, exquisite pain, etc., and frequently death." Almost every pathologist has copied the good Ambrose Paré, without perceiving that they very often cause what they forbid as dangerous to their patients; but M. Malgaigne has superadded to theory a strict practice.

The following tables exhibit the comparative mortality of childbed women in eight hospitals of Paris.

I. HÔTEL DIEU.

Year.	Confinements.	Deaths.	Prop. of Deaths.
1844	310	23	1 in 13
1845	405	19	1 in 21
1846	464	31	1 in 15
1847	398	14	1 in 28
1848	715	21	1 in 34

II. HÔPITAL DE LA PITIÉ.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	104	13	1 in 8
1845	117	10	1 in 12
1846	173	14	1 in 12
1847	148	17	1 in 9
1848	187	16	1 in 12

III. HÔPITAL ST. MARGUÉRITE.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	24	2	1 in 12
1845	31	1	1 in 31
1846	21	1	1 in 21
1847	17	0	0 0
1848	200	6	1 in 33

IV. HÔPITAL DES CLINIQUES.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	1135	41	1 in 28
1845	1221	44	1 in 30
1846	1119	42	1 in 27
1847	1300	31	1 in 42
1848	1160	24	1 in 50

V. MAISON D'ACCOUCHEMENTS.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	3907	160	1 in 24
1845	3758	139	1 in 27
1846	3975	148	1 in 26
1847	4227	133	1 in 31
1848	4080	110	1 in 37

VI. HÔPITAL NECKER.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	4	0	0
1845	3	2	2 in 3
1846	3	0	0
1847	7	1	1 in 7
1848	18	4	2 in 9

VII. HÔPITAL BEAUJON.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	34	2	1 in 17
1845	48	3	1 in 16
1846	70	5	1 in 14
1847	114	3	1 in 38
1848	212	8	1 in 26

VIII. HÔPITAL ST. LOUIS.

Years.	Confinements.	Deaths.	Prop. of Deaths.
1844	226	19	1 in 12
1845	295	11	1 in 26
1846	380	10	1 in 38
1847	396	7	1 in 56
1848	562	6	1 in 93

With the result shewn at the Hôpital Saint-Louis, during the last two years, M. Malgaigne has renounced in his treatment of Puerperal Metro-Peritonitis, baths, injections, leeches, and mercurial ointment, as the source of the mortality in other establishments, where the least statistical result which can be adduced is two per cent. of deaths.

His method is very simple, and does not exclude any indication. Like others, he opposes diarrhœa with opium, vomiting by *eau de Seltz*, and pain by cataplasms; but he alone takes the necessary precautions against chills, of which he stands in so great dread, as to have the bedding secured by ribbons from coming off the patient.

ALLEGED CAUSE OF STERILITY, DESCRIBED BY DR. VANONI, AND SAID BY HIM TO BE LITTLE KNOWN.

IN *Il Progresso*, a medical journal lately established at Florence, Dr. VANONI has published a paper on what he terms a little-known cause of Sterility. He alludes to a want of due development of the uterus, or atrophy coming on after the organ has acquired its proper volume. The author seems to have entirely overlooked the fact, that the former class of cases, are, when curable, simply cases of tardy puberty, and that the small uterus is simply the result of ovarian torpor continuing beyond the usual age. Uterine atrophy coming on from sexual excess, or other cause afterwards, is an interesting fact; but to speak of it as a cause of Sterility does not seem to be correct, because both it and the Sterility are the consequences of ovarian disorder. The disease of the ovaries, if slight or functional, may be curable by rest, moral discipline, and other means; but, if serious and organic, the diminution in the size of the womb, and the associated barrenness, may be beyond remedy. They may depend on lesions produced by inflammation of the ovaries. With this brief explanation of our views as to the proper aspect in which to regard Dr. Vanoni's paper, we give below an abstract of it from the *Gazette Médicale de Paris* for 17th November, 1849.

CASE. A woman aged twenty-one, who had been three years married, enjoyed excellent health, but remained sterile. Dr. Vanoni found the neck of the uterus high up in the vagina, of diminutive size, very hard, cold, and insensible. The orifice could hardly be felt, unless the finger were pressed against it with some force: the opening was circular. The base and the summit of the uterine neck were of equal dimensions: the central portion was slightly more voluminous, so that this expansion in the middle gave to the totality of the uterine neck somewhat the form of an olive. The body of the uterus, (as explored by having one hand in the vagina, and the other on the hypogastrium,) appeared to be very small, but of normal shape. The heat of the vagina contrasted strongly with the very low temperature of the cervix uteri. The mammae were somewhat less developed than they usually are at her age. Intercourse was destitute of sensual gratification, but did not occasion any repugnance. After ten years of sterile matrimony, she had an infant, in 1837; and afterwards two others.

Dr. Vanoni inferred from this case, that want of development of the cervix uteri is not necessarily incurable. Pursuing his investigations, he recognized the same characters of the cervix in a woman, aged forty, who, at nineteen had had a child, had then become addicted to libertinage, and had never again conceived. In this case, therefore, the uterine atrophy was not congenital. In several other women, married at such ages as thirty, thirty-two, thirty-eight, and forty, he recognized the same condition, to which he proposes to give the name of *hystero-trachelo-atrophy*, or *metro-trachelo-hysteria*. They did not become mothers, in spite of their anxiety to have children, and of the most assiduous performance of their conjugal duties. When these cases came under his notice, he was not able to find a remedy. In 1838, he was consulted by a lady aged twenty-three, who had been married for two years. Her menstruation was irregular as to time and quantity; and she presented the same conditions of the cervix as were described in the first case. She was devoid of sexual desire, sterile, and distressed at her sterility. Horse exercise and ferruginous waters were prescribed. In two years, the menstrual function had become natural, and the cervix a little larger, measuring from six to seven lines in length. In the following year, (1840,) the length had increased to thirteen or fourteen lines; and in 1842 to fourteen or fifteen lines. Its tissue was soft, and in some points spongy. As these changes progressed, the venereal appetite likewise went on increasing; and seven years after marriage, she was con-

fined of a boy. Two other analogous cases are detailed. In one, pregnancy occurred after nine, and in the other, eleven years after marriage.

Dr. Vanoni is of opinion that the natural stimulus, *i. e.* repeated sexual intercourse, is the best means of curing imperfect development of the cervix : of re-establishing its dimensions, normal texture, and functions, and thus giving an aptitude for fecundation. If, however, marriage be contracted too late in life, the imperfect development may become permanently arrested, and then what is usually efficient will be powerless. The author affirms, that if sexual intercourse be commenced before the patient have attained her twenty-ninth year, we may hope for the uterus becoming natural in size.

COINCIDENCE BETWEEN ANCIENT AND MODERN TREATMENT OF UTERINE DISEASES.

At p. 1030, in our review of Dr. H. Bennet's work, we gave extracts showing the antiquity of the SPECULUM ; and now, by the following extract from a paper in the *Provincial Medical and Surgical Journal*, of 27th June, by Mr. Ed. Copeman, of Norwich, it appears that the UTERINE SOUND and other reputed obstetric novelties are only re-discoveries, having been known two thousand years ago.

"As examples," says Mr. Copeman, "of the re-introduction of ideas and modes of practice in existence centuries ago, yet in the present day attracting all the attention of novelty, disseminated, too, by physicians whose position negatives the possibility of their practising dissimulation, we may adduce certain views lately promulgated with respect to torsions and flexions of the uterus, described as causes of disordered menstruation and sterility. Hippocrates, in his chapter on "Diseases of Females," describes these very diseases, as well as their effects and proper treatment. For instance, he attributes certain deranged states of menstruation to occlusion and distortion of the os uteri. 'Morbus fit, si uterorum os conclusum fuerit, aut *distortum*.' 'Si enim horum quid fuerit, menses exitum invenire non poterint priusquam uteri ad sanam naturam redierint. Aliquando dum ipsi uteri obtorquentur, etiam os ipsorum *distorqueri* contingit.' Again, he speaks of occlusion and distortion as causes of sterility, and advises the very same treatment said to be first recommended and practised by Dr. Simpson, of Edinburgh. 'Si os valde *conclusum* fuerit, *œdis et plumbeis fistulis immixtis aperito*. Quibus vero os *distortum* est, et ad coxam allapsum, per digitum avulsam à coxâ detrahito. Quum autem discesserit, *œdis et plumbeis fistulis immixtis*, juxta priorem rationem, *in rectitudinem dirigit*.' What is this but the uterine sound? Hippocrates also mentions the os uteri being open '*magis quam oportet*,' and its being thick and fleshy, as causes of sterility. Another cause he describes to be a membrane obstructing the os uteri, evidently not referring to the hymen, for his treatment consisted in destroying the membrane, by introducing lint covered with a stimulating application,—'*quam penitis-simè, filo ad extremitatem alligato*.'¹

"It is curious to observe how completely modern discoverers have been anticipated, and that the knowledge of these conditions of the uterus was almost as perfect (though probably not so diffused) two thousand years ago, as at the present time."

¹ Latin translation. Folio edition. "De Morbis Muliebris."
 Lib. 1, F 3 } page 370. Lib. 1, D 29-30, page 377-8.
 " G 4 } " C 35 " 379.

REPORTS OF SOCIETIES AND ACADEMIES.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

MONDAY, NOVEMBER 12, 1849.

DR. ADDISON, PRESIDENT, IN THE CHAIR.

CASE OF FOREIGN BODY IN THE RIGHT BRONCHUS. BY JOHN G. FORBES, M.R.C.S.—Mrs. W —, aged forty-six, applied to the Western General Dispensary, on May 11th, 1849, stating that on the previous day, whilst eating some broth, a small piece of solid matter, which she believed to be “bone, covered with gristle,” passed into the windpipe. She was immediately seized with spasmodic cough and threatened suffocation, and it was some minutes before she recovered herself. Twenty-four hours after the accident her voice was hoarse; respiration slow and wheezing; there was a constant short cough and pain in the upper part of the chest. On the right side the natural vesicular murmur was scarcely audible, and a prolonged and peculiar rhonchus was heard throughout the lung, but most distinctly over the point to which the pain was referred. On the evening of the third day, the patient was affected with considerable constitutional disturbance, ushered in by a long shivering fit. On the fifth day (May 14), when the author visited his patient, he was accompanied by Dr. M’Intyre, Mr. Arnott, and Mr. Anderson. A remission of symptoms had suddenly taken place; and it was decided that an operation should not then be performed. On the 16th, the cooing sound had degenerated into a peculiar whiff or puff. The expectoration was more copious, and the cough troublesome, and aggravated by the slightest exertion or excitement. Mr. Arnott again saw the patient, in consultation with the author and his colleagues; but as the symptoms of the patient were not very urgent, and it was very uncertain exactly where the foreign body was fixed, it was thought prudent not to operate. During the ensuing fortnight, the symptoms were, disturbed nights, paroxysms of fever, profuse night sweats, etc.; and the cough assumed a more paroxysmal character. On June 1st, the symptoms were further aggravated, the expectoration becoming more abundant. After this date, until June 21st, there was a considerable amendment in the general condition of the patient; but on the 22nd, the expectoration assumed a dingy appearance, and offensive odour; the breathing and pulse were hurried. On the 25th, the foetid, purulent expectoration was increased, and the constitutional disturbance further aggravated; and from this time she never rallied, but died on the 5th of July.—*Autopsy*: the carotid artery of the right side was observed to take an abnormal course, running obliquely upwards, and to the right side, in front of the trachea. The right lung filled its own side of the chest, and in its inferior two-thirds, was adherent to the ribs and diaphragm; and close upon the latter, in front, there was a pleuritic abscess, the size of the palm of the hand. The foreign body, a small piece of bone, weighing when dry three grains and a half, partly rough and sharp, and in part smooth, was found firmly impacted in the orifice of the third branch, given off from the bronchus, which passed into the middle lobe; a circumstance which accounted for the fact which had been noticed during life, that air passed with comparative freedom into the upper part of the affected lung. The lower two-thirds of the same lung were of an ashy slate colour, of dense consistence, very offensive odour, and infiltrated with a purulent fluid. That part of the upper lobe which was supplied with air appeared healthy. The author concluded by directing attention to the risk which would have been incurred, of wounding the right carotid artery, had an operation been attempted.

Dr. SIBSON said, that the foreign body in this case fell, as is usual and

natural in such cases, into the right bronchus, which is almost straight in direction when compared with the left. An important sign of the seat of the foreign body in such cases is afforded by the fact, that the movements of respiration of the parietes of the chest, over the obstructed part of the lung are lessened, absent, or even reversed, in proportion to the amount of obstruction. In this case, the movements of respiration were lessened over the affected part, while they were doubtless exaggerated over the unaffected part of the right lung, and the whole of the left. If a foreign body enter a bronchus, it will be easier for the air behind it to find its way out during expiration, than for the air to enter the obstructed bronchus during inspiration, the body being forced more inwards by the advancing column of air. The consequence must be, that the amount of air in the obstructed portion of lung will be lessened, and that portion will be diminished in size, and less resonant on percussion. The lessened or reversed respiratory movements, the diminished size of the chest, and the comparative dullness on percussion over the obstructed portion of the lung, taken in connexion with the auscultatory signs, and with the exaggerated respiration over the unaffected portion of lung, could scarcely, in any instance, fail to point out with accuracy the seat of the obstruction, when it was known that a foreign body had entered the trachea.

It had just occurred to him, that possibly a foreign body in one of the bronchi might be removed by the aid of a miniature air-tractor, like that invented by Dr. Simpson, placed on the end of a tube, converted into a syringe by means of a piston.

DR. C. J. B. WILLIAMS said, that the body was so small, and of so peculiar a figure, that it was scarcely to be expected that much physical indication of its presence would exist; and yet, in the beginning, peculiar signs did exist. Very few cases of a foreign body in the air tubes would occur, in which a careful examination of the effects of position, and of the state of the respiration, would not develope some physical signs of its presence. These signs, however, would vary with the size and form of the body itself. Thus, an angular body would rarely, if ever, produce such total obstruction as that mentioned by Dr. Sibson, and therefore that sign of itself was not to be regarded as characteristic in all instances. The pathological changes in such cases were rather those of irritation than of obstruction, and there would not be absence of respiratory murmur, nor would there be falling in of the chest. It was only round bodies that produced complete occlusion, as in the case related during the last session, in which a round pebble completely plugged up one of the bronchi, and where the respiratory sound on that side was absent. Generally, however, the foreign body was moveable, and here the sound would soon indicate its presence. With regard to the particular situation of a body in the right bronchus, this was generally more easily determined by examination of the back than the front of the chest, as the tube there was nearer the surface. The pathological changes first took place in the lung, at this part, and these changes illustrated the influence of a foreign body on the textures; at first there was irritation, pain, cough, and dyspnoea, often of a convulsive and spasmodic character, more constant and frequent than that which resulted from the body coming in contact with the glottis, and to which he did not allude. Smooth bodies would occasionally remain a long time in the bronchial tubes without producing any ill effects, but when rough, or like that observed in the present case, they soon gave rise to a peculiar kind of inflammation attended by a particular class of signs and symptoms. This inflammation passed into a chronic state, and was attended by a purulent, sanious, and even fetid discharge. Mr. Liston, indeed, had sometimes diagnosed the presence of foreign bodies almost entirely by this putrefactive smell, and in an unique case—which had been related by Mr. Humby¹ last session, and which he

¹ An account of this interesting case is given at p. 777 of the LONDON JOURNAL OF MEDICINE for August 1849.

regretted had not been published in the *Transactions*—a portion of ossified cricoid cartilage had come away by ulceration, and escaped into the bronchial tube; and though the signs of a foreign body in the air-passages were present, it was not at first suspected, as the larynx was much diseased. The body was expelled, by cough, through the opening in the trachea, however, some time after tracheotomy had been performed by Mr. Liston; but after death, a portion of cartilage was found in the opposite—the left—bronchus, having evidently become displaced after the operation. The pathological changes here were, as usual, indicated by irritation, foetid breath, etc., and consisted of consolidated lung, purulent infiltration, and low suppurative pneumonia. This case, among many others, was one exceedingly illustrative of the propriety of performing an operation for the removal of the foreign body as soon as possible after its presence was discovered. The evils of opening the trachea, he thought, were overrated, for he did not see much to apprehend, if it were resorted to before constitutional symptoms and morbid changes had presented themselves; but when bronchitis had supervened, the entrance of the cold air into the tube might be injurious. In the case before the Society, looking at all its details, he could not help thinking that an operation, if early performed, might have been successful; and looking at the results of non-interference in these cases, at the irritation extending deeper and deeper, at the inflammation and its natural consequences, he thought the operation should be resorted to, where practicable.

Mr. ARNOTT had seen this case five or six days after the foreign body was said to have escaped into the bronchus; and it was then a question whether it consisted of bone or gristle. Symptoms of the presence of a foreign body had existed, but at that time were absent—there seemed to be a lull in the case. Here was a woman previously ill with pain in her side, without any urgent symptoms, the foreign body probably gristle; certainly the case was not distinct enough in its features to warrant an operation. The foreign body was found after death in the third branch of the bronchus; but even if this had been known before, how difficult it would have been to remove a body, grasped as this was by, and filling, the tube. To get the forceps so low down would have been almost impossible, though they might be safely employed, perhaps, in the first or second division of the tube. The case was altogether different from that of Mr. Brunel, in which the foreign body was moveable. He looked upon this case as important, as having been minutely and accurately given; and if a precisely similar case should occur, looking at the certain results of non-interference, he should certainly operate, but with no great confidence of success. In this very case, however, the course taken by the carotid might have resulted, had an operation been performed, in the death of the patient on the table, from a wound of the artery; and it indicated the necessity of carefully examining the parts at the upper portion of the sternum, when about to operate in that neighbourhood.

WESTMINSTER MEDICAL SOCIETY.

NOVEMBER 3, 1849.

FRANCIS HIRD, ESQ., PRESIDENT, IN THE CHAIR.

ADJOURNED DISCUSSION ON CHLOROFORM. Mr. GREENHALGH stated the results of thirty-two cases of parturition in which Chloroform had been given, which had come under his observation. All the patients were at the full period. In three, the forceps were applied: of which cases, one mother died of puerperal fever five days after delivery: all the children were born alive. In one case, turning was had recourse to: the child was still-born. The remaining twenty-eight cases were natural labours. Eighteen of the children were females: fourteen were males. Hæmorrhage did not occur in any case: nor was there delay in the expulsion of the placenta. Two suffered from intense headache for some hours after delivery: one of the patients

had been subject to hysterical headache for years ; but the other had rarely had pain in the head, previous to the inhalation of the chloroform vapour. In no case did any permanent ill effects follow. The largest amount of chloroform made use of, in any one case, was two ounces and a half ; and this was extended over nine hours—the longest period of inhalation in any of the cases.

Lascivious dreams or remarks had occurred in only two out of a large number of cases in which Chloroform had been inhaled for various purposes. Mr. Greenhalgh concluded by giving it as his opinion, that although this agent occasionally produces dangerous and even fatal effects, yet if cases be judiciously selected, the remedy cautiously administered, and its effects properly watched, it may often be advantageously given both in natural and instrumental labours.

DR. HENRY BENNET had administered Chloroform, in obstetric cases, ever since its introduction by Dr. Simpson, and he was thoroughly in favour of its employment under certain circumstances. He had used it in three classes of cases.

First, in cases in which irritation of the system was kept up by fear or other causes, and the parturient efforts interfered with. In them, it allayed unnecessary pain, and quickened the labour. A young woman, in her first labour, had little energy, and seemed overwhelmed with pain and mental excitement. The head had advanced into the pelvis, but there was excessive irritability, with cerebral symptoms. The bad symptoms all gave way under Chloroform, and the pains became natural and expulsive. In such cases, where bleeding and opiates were formerly resorted to, he had found Chloroform much better. This medicine seemed to act directly on the cerebro-spinal system of nerves. The *second class of cases* were those in which operative procedure was necessary ; not only did the Chloroform relieve unnecessary pain, but facilitated the efforts of the accoucheur in delivering his patient. In *simple parturition*, he did not give Chloroform, except at the request of the patient. In no case had he seen ill effects fairly attributable to Chloroform. In one instance a lady died in child-bed three weeks after delivery, under the influence of Chloroform ; but the fatal result was dependent on an organic disease of the heart not discovered during life. In one case, also, some slight *hæmorrhage* had occurred ; it was easily arrested, and was not due to the medicine. The *third class of cases* were those of inflammatory disease of the uterine neck, in which it was necessary to apply caustic, or to operate ; it relieved pain, fear, and neuralgia, and was most valuable, as it was also after operations in relieving pain. He had never seen it give rise to indecent talking ; on the contrary, he had seen it have a temporary good effect in cases of nymphomania.

MR. W. F. BARLOW thought that some persons made too light of pain, and underrated its complex effects upon the body. Let them turn to Mr. Travers's work on *Constitutional Irritation*, and there read of deaths from the shock of pain. Pain had remote effects as well as immediate, and the former were apt to be forgotten. The remark was quite applicable to the pains of parturition. As to the question, Can Chloroform be safely administered in labour ? it was one which facts only could determine.

He had heard it said, that the production of anæsthesia in labour had shown that the action of the uterus was independent of the spinal cord. It showed nothing of the kind. How could it be said that the influence of the cord was withdrawn from the uterus, so long as respiration continued ? Was it possible to destroy the function of the cord below, and yet leave the medulla oblongata active, there being only one circulation for the whole cord ? He had repeated, since last meeting, one of Dr. Tyler Smith's experiments. He had poisoned a Guinea-pig with the vapour of Chloroform, and found that no contraction of the muscles followed the irritation of the spinal cord, not even when a wire was suddenly thrust down the whole length of the vertebral

canal; the muscles were, however, very responsive to the action of galvanism.

MR. GREAM had tried both ether and Chloroform, to some extent, when they were first introduced; and had satisfied himself that, when given in the full doses, and for the long periods then in vogue, they were injurious and dangerous. Dr. Murphy had adduced no new facts; and he would ask Dr. Murphy why he had not quoted Meigs, Montgomery, and the like; and why he had not cited any London accoucheur in large practice, but had only referred to authorities of less weight? Mr. Gream maintained that chloroformists had now come to his way of thinking, as they owned that they did not now inebriate with the drug. Perhaps few gave it to any extent sufficient to influence the patient injuriously; and after this fashion he would allow his patient to inhale the vapour, though he would never offer it. There was neither danger nor the chance of danger in the present "pretended anæsthetic midwifery", which was chiefly an effect on the mind. Mr. Gream argued against the analogy between parturition in the sow, and in the human female. He stated that the removal of a portion of the spinal marrow, or the crushing of it, would not impede labour so much in the former as in the latter, who from the obliquity of the axis of the pelvis, and the greater comparative bulk of the young, (with reference to the outlet,) required the more complete assistance of the spinal system of nerves. Mr. Gream had observed the phenomena of parturition in pigs, and had noticed that there was very little voluntary expulsive effort. Primiparous women required the full benefit of the reflex spinal system, and of the voluntary muscles; and in them, Chloroform inhalation must retard parturition; but Chloroform must exert some control even over the ganglionic uterine action, or the action depending on the peristaltic movements of the uterus, as muscles only possessed the integrity of their power when supplied with good blood; but persons drugged with Chloroform had black and bad blood. Mr. Gream entered into a variety of topics illustrative of the injurious effects of complete anæsthesia in midwifery, and of the harmlessness of the pretended anæsthesia, which latter, he said, had supplanted the former.

DR. WEBSTER detailed three cases which had come under his cognizance (in Bethlem Hospital), showing the serious consequences sometimes following the inhalation of Chloroform during child-birth. In the first case, the patient was for three days incoherent. She soon afterwards became so furious, as to require confinement. After twelve months, she was discharged cured. In the second case, the patient never recovered from the effects of the Chloroform, and soon after delivery became quite maniacal, and continued so for many months, but recovered ultimately. The third case might not be considered as a true instance of insanity: however, he would relate the chief symptoms. The cerebral disturbance, following the use of Chloroform, never ceased entirely; the patient could not sleep at night, and often said she felt as if in the presence of a madman who was going to murder her. Three weeks afterwards, she became almost maniacal, exhibited much mental excitement, laughing frequently; conducted herself like an infant, and lost her memory, in which state she continued during five months, when recovery took place.

DR. MURPHY replied. He was anxious only for truth, and was glad to hear any well-authenticated facts on either side of the question. Doubtless there were some peculiar constitutions, in which Chloroform could not be given without ill effect, as was the case with opium, calomel, etc. To determine what were these constitutions, in what way to administer the Chloroform, and to determine its real value, was, and should be, the object of his inquiries.

NOVEMBER 10, 1849.

DR. SNOW, V.P., IN THE CHAIR.

REMOVAL OF A TUMOUR IMBEDDED IN THE PAROTID GLAND. MR. CANTON

related the following case: A man, æt. 60, had for several years been afflicted with amaurosis of both eyes, for which mercury, strychnine, galvanism, and other remedies, had been ineffectually employed. Some time since, he complained of a swelling, below the left ear, which had gradually increased. It gave no pain; but by degrees the resulting inconveniences were, difficulty in mastication and deglutition, and great impairment of hearing. The tumour was firmly wedged in between the ramus of the jaw and the mastoid process, extending chiefly towards the angle of the former. The skin over it was healthy and not adherent, and to the touch it was firm and resisting. On removal, it was seen to be an enlarged and apparently fungoid lymphatic gland, inclosed in a strong, fibrous-looking capsule. The hæmorrhage was trivial, and the patient recovered without any paralysis of the face, and with restoration of the power of mastication, deglutition, and hearing.

MR. CHIPPENDALE said that Mr. Canton's case was another illustration of the truth of the well-known surgical axiom, that the parotid gland could not be removed by surgical procedure. Often a lymphatic gland, on removal, was mistaken for the parotid itself.

LARGE TUMOUR, WEIGHING NEARLY THREE POUNDS, REMOVED FROM THE FACE AND NECK OF A MAN, AGED FIFTY-SIX. By MR. HAYNES WALTON. A cast, taken before operation, was produced. The disease had existed for seven years. The tumour, also exhibited, had a cavity in its centre. Microscopically, according to Dr. Routh and Dr. Peacock, it presented a fibro-cartilaginous structure. The patient made a good recovery, and returned home in fourteen days.

NOVEMBER 17th, 1849.

FRANCIS HIRD, ESQ., PRESIDENT, IN THE CHAIR.

PULMONARY TUBERCLES IN A VERY YOUNG CHILD. DR. ROGERS exhibited the lungs of a child, not four months old, who had died of tubercular meningitis, (*hydrocephalus acutus*;) they were infiltrated with tubercle in every stage and form, miliary, grey and yellow, large masses of which, when cut into, were found entirely softened. The bronchial glands were greatly enlarged, and infiltrated with tubercle. The mother was phthisical, her relatives had died of phthisis. The infant, at birth, was a fine, large child, apparently healthy, but evidences were quickly developed of a strong constitutional taint, amongst which was cough, continuing during its whole life.

NEW INSTRUMENT FOR APPLYING CAUSTIC SOLUTIONS TO THE URETHRA. MR. H. SMITH exhibited this instrument. It consisted of a catheter with a long stilette, to the end of which was attached a spongy body of the same calibre, which could be thrust out of the tube at any part of the urethra. The sponge was saturated with the fluid, and thus applied.

DIAGNOSIS AND TREATMENT OF CHRONIC DISEASES OF THE SKIN. MR. HUNT endeavoured to prove, that little or nothing could be learned of the nature or treatment of these diseases from the mere forms of eruption; and that the variations of these appearances, were little more than the different modes in which inflammation of the dermis terminated, any of which forms might arise, respectively, either from specific irritation, as in syphilis, or from general cachexia; that the origin was not distinguishable in the form of eruption, both causes inducing various kinds of eruption; that the syphilitic cases must be diagnosed by their history rather than by their hue, and that the evidence of primary affection was often obscure or wanting.

The author drew a distinction between skin disease resulting from contagion, or other external sources of irritation, in which external applications might be useful, and those diseases arising from within, in which external applications could not cure, but might do harm. He described the management required for the various complications of the eruption, such as febrile

action, functional or organic visceral diseases, anæmia, plethora, etc. And lastly, he described the treatment required for the simple spontaneous diseases of the skin, of whatsoever form or appearance, in all of which he had found a judicious and persevering course of arsenic the great remedy.

The medicinal action of arsenic was portrayed in four degrees, in the *first* of which it acted simply as a tonic, improving the appetite, invigorating the strength and spirits, and warming the extremities; in the *second* degree, exciting the mucous membranes and the skin; in the *third* degree, acting on the serous membranes, and exhausting the strength; in the *fourth*, showing its toxic effects. The first two degrees were described as salutary; the third, injurious; and the fourth, dangerous. In small doses only it acts usefully as a medicine, and the dose should never be increased, but diminished, not discontinued, when the conjunctiva becomes affected. Five minims of Fowler's solution was the maximum dose, which the author always administered on a full stomach.

An interesting discussion ensued, in which Drs. Garrod, Lankester, Murphy, Sibson, Routh, Snow, and others took part.

APPOINTMENTS.

BENNETT, J. Risdon, M.D., appointed Physician to St. Thomas's Hospital, in the room of the late Dr. BURTON.

CORBETT, Dr., elected Professor of Anatomy to the Apothecaries' Company of Ireland, in the room of Dr. ALCOCK, appointed to Queen's College, Cork.

CRISP, Dr. Edwards, elected Physician to the Metropolitan Dispensary.

GARROD, A. B., M.D., appointed Professor of Materia Medica and Therapeutics in University College, London, in the room of the late Dr. A. T. THOMSON.

HEALE, Dr., elected Physician to the Royal Free Hospital, in the room of Dr. PEACOCK.

KIRTON, Percival, Esq., appointed Colonial Surgeon for the Settlements on the Gambia.

PEACOCK, T. B., M.D., appointed Assistant-Physician to St. Thomas's Hospital, in the room of Dr. J. R. BENNETT.

SMITH, Dr. R., elected by the Board of Trinity College, Dublin, Professor of Surgery in the School of Physic.

OBITUARY.

BRICE, W., Esq., late Assistant-Surgeon of H. M. S. Blazer, aged 32, on 29th October.

CHARLES, Hugh, Esq., Surgeon R.N., at Fisher Row, near Edinburgh, on 23rd October.

DOBBIN, W. W., Esq., Surgeon, at Newtownhamilton, on 1st November.

FROLIOTT, William, M.D., at Clonakilty, Ireland, aged 59, on 16th October.

GRAZER, George R., M.D., at Portarlinton, Ireland, on 25th October.

FRAHAM, Henry, M.D., of Edinburgh, at 2, Maitland Place, Newhaven, on 1st November.

GRANT, Samuel, M.D., formerly of the Honourable East India Company's Service, at 121, Sloane Street, of Cholera, aged 71, on 13th November. He served most zealously for twenty-five years, and was present in many severe battles.

HARTWELL, J. L. Esq., Surgeon, 2nd class, at Sydney, New South Wales, on 12th June.

LLEWELLYN, C., M.D., Surgeon, 40th Native Infantry, at Barrackpore, on 9th September.

MACKEY, George, M.D., at Adare, county Limerick, lately.

MORTON, Thomas, Esq., Surgeon to University College Hospital and to the Queen's Bench Prison, at his residence, Woburn Place, Russell Square, aged 35, on 30th October. This talented surgeon and most estimable man, died from prussic acid, administered by his own hand during a fit of insanity, connected with hypochondriasis.

OLDAKER, Francis Allcock, Esq., Surgeon, at Brinklow, aged 30, on 4th November.

ORPEN, T. H., M.D., at Queenstown, Ireland, on 3rd November.

THOMSON, George, M.D., Assistant-Surgeon, 3rd Brigade Horse Artillery, at Lahore, of apoplexy, on 27th August.

WOODYER, Caleb, Esq., Surgeon, at Guildford, Surrey, aged 83, on 5th November.

WORRALL, Joseph, M.D., at Limerick, on 6th November.

BOOKS RECEIVED.

DOWNING on Painful Affections of the Nerves. London: 1849. KNOX (Dr. Alexander), on Cholera. Dublin: 1849. LUNIER, Recherches sur la Paralyse Générale et Progressive. Paris: 1849. REPORT OF COLLEGE OF PHYSICIANS on the Cholera Fungi. London: 1849. HOSPITAL FOR CONSUMPTION, First Medical Report of. London: 1849.

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